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BEE PARASITES.—IV.

By E. A. Butler.

(*Continued from page* 167.)

TO conclude our papers on the parasites to which British wild bees are subject, it yet remains to consider the most extraordinary of them all, the extremely aberrant beetles belonging to the family *Stylopidæ*. It will be best to introduce these remarkable insects to our readers as they were in the first instance introduced to the British scientific world, and in fact, one may almost say, to the scientific world at large. In the year 1802 the Rev. W. Kirby, Rector of Barham in Suffolk, well known as the joint author with Spence of the "Introduction to Entomology," published an important monograph on British bees, a group of insects to which he had devoted much study, and which he was the first to properly classify and describe. It was while collecting materials for this work that he came across the first *Stylops* ever recorded to have been seen by any English naturalist, and in the above monograph he inserted a description of his discovery in a passage which has since become classical to the student of the *Stylopidæ*. With the acuteness of observation which distinguished him, he had noticed minute protuberances on the bodies of certain bees. These, he thought, were possibly mites of some kind, as he well knew that such creatures are often found on bees' bodies; but whatever they might be, the mere fact that there was something of a mystery was quite sufficient to impel him to inquire into the matter and endeavour to get at the truth of it. He was thus led to his great discovery, but what follows had better be told in his own words. In reference to his examination of the first of these little knobs, he says, "What was my astonishment when, upon attempting to disengage it with a pin, I drew forth from the body of the bee a white fleshy larva, a quarter of an inch long, the head of which I had mistaken for an Acarus." This was half the discovery, but we shall see presently that Kirby was mistaken in supposing that the maggot-like creature he had extracted from the bee's body was a larva; it was, on the contrary, a perfect insect, being in fact the female *Stylops*. For many years, however, it was imagined, and very naturally, considering its form, to be a larva; but further facts as they were discovered were seen to be more and more difficult to reconcile with such a supposition, and at last it was recognized that the footless grub-like creature was the last stage in an eventful history, a mature insect of an extremely degraded type.

But, as we have said, this maggot-like grub was only half the discovery, and that the less romantic half. Mr. Kirby continues: "After I had examined one specimen I attempted to extract a second, and the reader may imagine how greatly my astonishment was increased when, after I had drawn it out but a little way, I saw its skin burst and a head as black as ink, with large staring eyes and antennæ consisting of two branches, break forth and move itself briskly from side to side. It looked like a little imp of darkness just emerging from the infernal regions. I was impatient to become better acquainted with so singular a creature. When it was completely disengaged, and I had secured it from making its escape, I set myself to examine it as carefully as possible, and I found, after a careful inquiry, that I had not only got a nondescript, but also an insect of a new genus, whose very class [*i.e.* order] seemed dubious." This was the other member of the *Stylops* family, the active little male fully equipped with wings and legs, and as great a contrast to his mate as could possibly be imagined. Mr. Kirby had been extremely fortunate in securing both sexes on the same occasion, for the males are far less numerous than the females; F. Smith estimated that the usual proportion was about one male to twenty females. Of course, Kirby did not recognize the two creatures as thus related to one another, but looked upon one as the larva and the other as the perfect insect, never dreaming that there would be such discrepancy in the forms of the adult insect. As he did not know anything of the life-history of his nondescript, he failed to see any close resemblance between it and any of the types of the usually recognized orders of insects: he therefore proposed to put it in an order by itself, or at least accompanied by another very similar insect, which had just been discovered by a continental observer in the body of a wasp, and had been thought by him to be a kind of ichneumon fly. This new order was called Strepsiptera (twisted wings) in allusion to the curiously bent or twisted form of the rudimentary fore wings. When, however, its life-history came to be unravelled, it was seen that there were strong resemblances between *Stylops* and *Meloë*, so that the most natural place for its family was evidently amongst the Coleoptera, near to the oil beetles. There it and its few relatives which have been thus far discovered are therefore at present resting.

Let us now look a little more closely at our two insects, and then we may enquire how they came to be imbedded in the bee's body in which they were found. And first as to the male (Fig. 9). There are several species of

Stylopidæ, but they are all very minute creatures, the largest being not more than a quarter of an inch long, while the rest are considerably smaller. The species in question has a jet black body and a pair of large milk-white wings. Its eyes are large and staring, appearing almost as if perched on the end of stalks, but the facets of which they are composed are very few, not more than about fifteen to each eye; the antennæ, most unusually, are each composed of *two* branches side by side. The fore wings are reduced to extremely minute dimensions, and bent as above described; what their use can be is quite conjectural. The hind wings, on the other hand, are ample, each spreading, when fully extended, over about a quadrant of a circle. Connected with this great development of hind wings is the large size of the hinder portion of the thorax, to which the wings are attached, and which, therefore, lodges the muscles by which they are rapidly vibrated. When not in use, the wings are folded longitudinally and laid along the back. The body is small and weak and, when the insect dies, soon shrivels up. F. Smith says, "The texture of all parts of the body of a male *Stylops* is of so delicate a nature that within two hours after death the entire appearance of the insect is changed, bearing no nearer a resemblance to the living creature than a shrivelled mummy does to the once manly and graceful Egyptian."

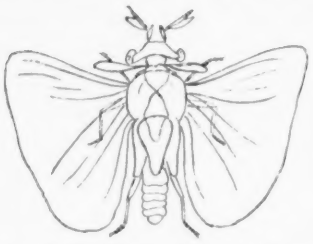

Fig. 9.—Male *Stylops*. (After Westwood.)

The little creature lives apparently but a few hours, and during that time takes no food, though it has a pair of jaws, being intent upon its sole business, the discovery of a mate. But in the pursuit of this business it manages to squeeze a good deal of activity and movement into its brief existence. As may be imagined, it is a by no means frequent occurrence to see these ephemeral beings on the wing, and only a few such opportunities have been recorded. Dale describes a *Stylops* as flying "with an undulating or vacillating motion amongst the young shoots of a quickset hedge," and adds, "I could not catch it until it settled upon one, when it ran up and down, its wings in motion, and making a considerable buzz or hum. . . . . I put it under a glass and placed it in the sun; it became quite furious in its confinement, and never ceased moving about for two hours. . . . . It buzzed against the side of the glass with its head touching it, and tumbling about on its back." Thwaites, again, speaks of them as being "exceedingly graceful in their flight, taking long sweeps as if carried along by a gentle breeze," usually flying so high as to be out of reach, but "occasionally descending and hovering a few inches from the ground. . . . . When captured they are exceedingly active, running up and down the sides of the bottle in which they are confined, moving their wings and antennæ very rapidly."

The search for a partner cannot be a very easy matter, seeing that the destined bride is completely imbedded in the body of a bee, and no part of her is visible outside except the minute knob-like fore-part, and this must be quite unnoticeable while the bee is on the wing. Hence, this matrimonial hunt must be an exciting occupation, especially as the hours pass by, and the sands of the hunter's little life are running rapidly down. No wonder, therefore, that its senses are preternaturally quickened, as we conclude to be the case from the fantastically complicated antennæ, and the prominent eyes, so placed as to take in the whole horizon at a sweep.

The female is shaped something like a phial bottle, the narrow part being connected with the broader by a still narrower constriction. The small anterior part consists of the head and thorax fused into one, and it is a portion of this that projects between the segments of the bee's back. The rest of the creature lies buried in the bee's abdomen in the manner shown in Fig. 10, occupying, sometimes, as much as one-fifth of the whole space. As regards organization, the condition of this curious being might be not inaptly expressed in the words with which Shakespeare closes his description of the seven ages of man:

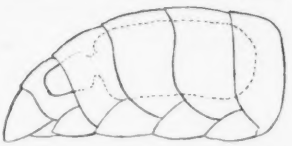

Fig. 10.—Outline of female *Stylops* in abdomen of Bee. (After Paskard.)

"Sans teeth, sans eyes, sans taste, *sans everything*."

She has neither legs nor wings, and never leaves the body of her host. Such food as she takes appears to enter her body by absorption from the surrounding tissues of the bee, and her abdomen becomes simply a chamber for the development of the eggs and the hatching of the young. She produces an immense number of larvæ viviparously, which, as soon as hatched, escape through an aperture in her thorax, which is the natural orifice of the reproductive organs transposed from its usual position to the only spot in which it would be of any use. The hatching of the larvæ is followed by the death of their parent. The larvæ are extremely minute, being not more than $\frac{1}{46}$th of an inch in length; they swarm in hundreds over the body of the bee, clinging to its hairs, and giving it the appearance of being covered with dust. From Fig. 11 it will be seen that the larva in its first condition bears not the slightest resemblance to either of its parents, being an active six-legged creature with long hair-like appendages at the tail, just as is the case with the newly-hatched larva of the oil beetle. It has, however, no claws at the end of its feet. The bee seems to object to the presence of the larvæ on its body, and their emission apparently causes it considerable annoyance and irritation, and renders it very excited. A stylopized female of *Andrena Trimmerana* was once kept for some days by F. Smith, enclosed in a gauze-covered box, and continually kept supplied with fresh flowers. One day he noticed the bee "running about apparently in a very excited state, burying herself beneath the leaves and flowers, then issuing forth and running round the sides of the box; sometimes she would stop, bury her head in the petals of a dandelion, and then commence brushing herself with her posterior legs, passing them quickly over the upper surface of the abdomen." Closer examination showed that she was covered with *Stylops* larvæ, which she was doing her best to get rid of. No doubt large numbers of the larvæ perish in this way, being brushed off and falling into situations in which they get no chance of mounting on bee-back again.

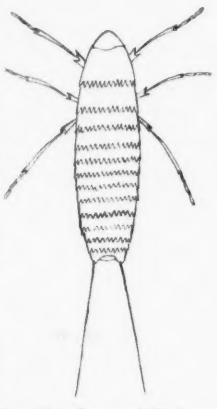

Fig. 11.—Larva of *Stylops*. (After Newport.)

Like the larvæ of *Meloë*, it is now essential that the young *Stylops* should be conveyed to the cell of a bee, since

its next move must be to enter the body of a bee larva, which will thus be selected as its lifelong host. Now here arises a very nice question; in order to take this step in its advancement, will it avail the *Stylops* larva to remain on the body of the bee that produced it, or is it necessary that it should leave its foster parent and seek out an unstylopized specimen? This evidently turns again on the question whether the stylopized bee is capable of reproducing its kind, a question somewhat more complex than might at first be imagined, and in our endeavours to answer it we must consider for a while what effects the parasitism of the *Stylops* produces on the bee. These are very considerable, and may be classified into two groups, internal and external effects. The internal effects run chiefly in the direction of the reduction in size of the various organs of the bee's abdomen. It is obvious that the presence of so large a foreign body amongst the bee's vitals must produce pressure upon surrounding parts and interfere with their proper growth and development. Thus the digestive tract of the bee is lessened in size and its walls become thinner, whereby the fluids it contains more easily pass through into the body of the parasite. But the effect is most marked in the case of the reproductive organs, which are much reduced in size and retarded in development. Both sexes of the bees are liable to the attack of the parasite, though the females, as a rule, seem to be the more frequently selected. The exact amount of interference with the bee's well-being which the parasite produces varies a good deal, according to the species of bee, the sex of both parasite and host, and the number of parasites present, which may be as much as four or five in one specimen, though most frequently they are solitary. Dissection of the bee's body has, therefore, led to somewhat conflicting results, some observers chronicling the almost entire abortion of the reproductive organs and the entire absence of their characteristic products, and others noting only their reduction in size and retardation in development.

But even supposing the female stylopized bee to be able to produce fertile eggs, this is by no means all that is necessary to ensure the production of a brood. The young bee grub must be provided with food, and to this end pollen must be collected, and this cannot be done in the absence of the pollen-collecting organs. This leads us to consider the external changes produced by stylopization. The exact details here again will differ in different cases. Our present business is with the female bee, and we will therefore consider only the changes produced in the external appearance of a female bee of the genus *Andrena*. These have been very clearly pointed out by Prof. Perez in a communication made to the Linnæan Society of Bordeaux, and his observations have been confirmed by other observers. The head of the bee becomes smaller and the abdomen more globose, while the hairs tend to become less densely distributed and paler in tint, and to form bands at the edges of the segments. The two chief parts of the pollen-collecting apparatus in an *Andrena* consist of a brush of hairs on the outside of the posterior tibiæ, and a little curl or lock of rather long hairs on the posterior trochanters, the minute joint which is situated between the basal joint which attaches the leg to the body and the thigh. Now in the stylopized bee these masses of hair become shorter, and therefore less fitted for the collection of pollen. In fact, in most respects the female bee approaches the condition of the male, while the stylopized male, on the other hand, tends to assume the characters of the female. M. Perez, who has examined great numbers of these bees, says that he has only met with *one* single specimen of a female stylopized *Andrena* which was carrying pollen on its collecting brushes. Now if it be the case, as is generally believed, that a bee does not collect pollen till it is fertilized, this would seem to indicate that stylopized bees do not become fertilized. If this be so, then, evidently, the only chance that the newly-hatched larvæ, which we left scrambling about amongst the forest of hairs on the body of their foster parent, have of further development is to dismount their present steed and seek another which is not thus handicapped; and it is clear that such a step will, under any circumstances, be absolutely necessary in the case of those produced from *male* bees. This is evidently, then, a dangerous and critical period in the little creature's career, and amid its many risks no doubt vast numbers perish, just as do the *Meloë* larvæ. That such must be the case is indeed evident from the contrast between the multitudes of larvæ produced and the comparative scarcity of the perfect insect.

Suppose, however, that a larva has successfully gained the back of an unstylopized female bee, and is by her conveyed to the cell she has made and provisioned for her own larva. The *Stylops* larva now dismounts, and in some way insinuates itself into the body of the bee grub, subsequently casting its skin and losing its legs, becoming itself maggot-like, and thus adapted to the new phase of life upon which it has entered. In this larva it remains, growing with its growth, and passing through its own metamorphoses as its host does, so that it becomes matured at the same time that the bee puts on its final form. During its preliminary stages it lies with its head towards the head of the bee, the position in which it made its entrance into the bee's body, but previously to maturing it reverses its position, and thus is enabled, if a male, to make its exit into the outside world head first, just as Kirby witnessed it, or if a female, is placed in a suitable position for fulfilling the functions that yet remain for her before she can give birth to her enormously multitudinous family. Having thus followed the course of the development of the *Stylops* throughout its life-history, we see that it does not, as so many internal parasites do, cause the death of the individual host, though, if the bees are really rendered abortive by its presence, it tends to the extinction of the *species*.

Stylopization, in some cases, so greatly alters the bee's appearance as to have led to the erroneous creation of pairs of species, the stylopized form having been described and named as a distinct species from the bee in its natural condition. Besides the sexual reversals already enumerated, there is one very curious result of stylopization for which it is difficult to find a physiological explanation; this is the alteration that takes place in the colour of the face in some bees. There are certain bees of the genus *Andrena*, notably *A. labialis*, in which the face of the male is yellowish white, while that of the female is blackish. Stylopized specimens of such species, if they are males, tend to have the pale parts reduced in size and altered in shape, while the females show a tendency to appear with spots of yellow on the usually dark face.

Erratum.—In Mr. E. A. Butler's article, "Bee Parasites," on page 164 of our last number, the inscription under Fig. 8 should read, "Stages of larva of *Sitaris muralis*. (After Fabre)."

THE FUEL OF THE BLAST FURNACE.

By Vaughan Cornish, M.Sc., F.C.S.

In a former article (Knowledge, February, 1892) we traced the progress of our knowledge of the element carbon, from the time when it was recognized as a reducing principle by means of which ores could be made to yield the metals, to the time when Lavoisier

showed that in this process of reduction the element carbon abstracts the oxygen from the ore, forming carbonic acid. It was pointed out, also, that in such *reducing* processes the carbon plays a two-fold part, for while one portion unites with the oxygen of the air, and in this process generates heat, the high temperature thus brought about enables the remainder of the carbon to abstract the oxygen from the ore, forming more carbonic acid, and leaving the metal. It is merely a matter of arrangement and of disposition of the parts of apparatus whether one portion of carbon is used as the heating, and a separate portion as the reducing agent, or whether the two processes are carried on simultaneously in the same parcel of the material. The former condition obtains, for example, when a powdered metallic oxide mixed with finely-divided charcoal is placed in a glass or porcelain tube, which is heated in a charcoal furnace. The second condition would obtain when the common hæmatite iron ore (an oxide) was mixed with charcoal in the old-fashioned iron furnace of Spain, and the charcoal having been set alight, and the heat having been urged by a blast of air, the iron was reduced to the metallic state by the action of the carbon.

In any process of iron smelting in which an oxide of iron is reduced by some form of carbon the initial and the final states may be represented thus :—

Oxide of iron + carbon = metallic iron + carbonic acid.

The symbol = must here be interpreted "may be caused to yield." But as our acquaintance with a chemical process becomes more intimate we generally find that a knowledge of the initial and final states of matter is not sufficient either for a proper understanding of what goes on, or, in many cases of manufacturing processes, for regulating the process so as to obtain a good "yield" of the valuable product. For this it is often necessary to learn the steps between the initial and final stages, even if the "intermediate" bodies formed have but a transitory existence. This is especially the case in the reduction of iron ore in the blast furnace. The modern improvements which enable manufacturers to keep pace with the demand for iron roads and iron ships are based upon the conditions of formation and decomposition of the lower oxide of carbon, carbon monoxide gas, which is formed either in the incomplete combustion of carbon, or when heated carbon acts upon carbonic acid. Carbon monoxide gas readily takes fire in presence of oxygen, burning with formation of carbonic acid. In so doing the carbon takes up a second dose of oxygen, being combined in carbonic acid with just twice as much oxygen as in the lower oxide. Carbon monoxide also acts as a reducing agent, the gas at a high temperature extracting the oxygen from metallic ores.

If, in any metallurgical process a portion of the coal, coke, or charcoal used is only burnt to carbon monoxide, then, in respect of that portion, we only get a part of the heating effect of the fuel, and half the reducing effect of the reducing agent. True, the hot carbon monoxide issuing from the apparatus burns in the air, forming the final product, carbonic acid, and producing more heat; but we want that heating effect in the apparatus, not outside it, and we wish, if it be possible, that the extra dose of oxygen required for complete combustion of the monoxide should come from the ore and not from the atmosphere.

In the blast furnace the materials put in at the top are oxide of iron, coke (*i.e.*, *charred* coal), and limestone. The lime of the limestone combining with siliceous matter mixed with the ore removes these impurities in the form of a light fusible slag, which floats upon the top of the liquid iron collected at the bottom of the furnace. The air is supplied in a forced blast through tubes called twyeres, placed near the bottom of the furnace. The furnace is always kept full, or nearly full, and we may consider the conditions at any part of the furnace as remaining constant, as, when once lighted, the furnace is kept always in operation till it is "blown out," after perhaps twelve or fourteen years' work. The furnaces used at the beginning of the century were about forty-eight feet high. Those used now are eighty feet. It was found that by giving more time to the ascending current of carbon monoxide a greater proportion of ore was reduced. A great deal of the gas in the smaller furnace escaped without, so to speak, having the chance of becoming fully oxidized.

A still more important improvement was the introduction of the *hot blast*. It appears paradoxical that one ton of coke burnt to heat the air before entering the furnace should serve three tons of coke in the furnace itself; yet so it is. The heating (with hot blast) being partly done from outside, it is not necessary to put so much coke into the furnace; consequently, there is room for more of the ore. The power of the ore and limestone to intercept heat is double that of the coke which has been replaced, and there is a greater surface of the ore to act on the diminished and slower current of carbon monoxide. Hence, greater economy in the production of pig-iron was effected by the use of the hot blast, and by raising the height of the furnace to eighty feet. Some furnaces were built of one hundred and three feet, but it was found that no further economy was effected. The present approved dimensions of furnace and temperature of blast appear to give a better yield of metal than if the furnace be larger and the blast hotter, and a better yield than if the furnace be smaller and the blast colder. The reason is as follows:—We have together in the furnace, carbon monoxide and iron ore, carbon dioxide and coke. If the heat contained in the carbon monoxide is more completely intercepted before passing out of the furnace, then, as we have seen, more of the ore is reduced and more of the monoxide is burnt to dioxide. But, on the other hand, if the temperature of the carbon monoxide be still further increased by the use of a still hotter blast, and if the heat of the up-flowing carbon monoxide be yet more completely intercepted before the gas issues at the mouth of the furnace, then we find that the temperature of the other materials is raised so far that the incandescent coke reduces the carbonic acid to carbon monoxide as fast as the ore oxidizes the monoxide to carbon dioxide. Consequently we have two opposite reactions which balance one another under certain conditions. When the temperature rises so much that the reduction of carbon dioxide is more rapid than the oxidation of carbon monoxide, it is obvious that we have passed the point where the furnace works most economically, for the reduction of the carbonic acid is accompanied by an absorption of heat. It appears therefore, as the result of practical experience, that we must always be content to only half burn in the blast furnace a large proportion of the fuel, sending out a *mixture* of carbon monoxide and of carbon dioxide from the mouth of the furnace.

One of the first great improvements in blast furnace practice was that of heating the blast. A more recent improvement has been to utilize outside the furnace the burning of that proportion of the carbon monoxide the presence of which in the escaping gases is, as we have shown, a necessity. In the modern furnaces the mixed carbon monoxide and carbon dioxide are drawn off near the top of the furnace, instead of being allowed to come in contact with the air. The hot gases are only admitted to the presence of oxygen when they have been brought by pipes to the furnace where the air for the blast is heated. The combustion of the carbon monoxide not only heats

the air for the blast, but furnishes power sufficient to drive the blast engine. The arrangements for utilizing the formerly "waste" gases of the blast furnace have effected a saving of more than half the total heating power of the coke, a saving equal, in Great Britain alone, to about four million tons of coal per annum.

GRASSES.

By J. Pentland Smith, M.A., B.Sc., *Lecturer on Botany at the Horticultural College, Swanley.*

THE exceedingly natural group of plants known as grasses constitutes a large part of the flora of the British Isles, and is found generally diffused over the surface of the globe, "from the utmost limits of phænogamous vegetation towards the poles, or on alpine summits to the burning plains of equatorial Africa." Some species arrest the traveller's attention by their wealth of numbers, some by the commanding height which they attain, and others by both features combined.

They may live for one, two, or many years; some spread by means of underground stems, which send out roots here and there to fasten the plant in the soil and obtain fresh supplies of nourishment. In this way, certain species become troublesome weeds, as the well-known couch grass (*Agopyrum repens*). The roots vary much in size; in this country, from compact tufts, as in the vernal grass (*Anthoxanthum*), to the enormously developed vertically descending shoots of the wheat, which find their way many feet down into the soil. The root of the embryo soon ceases to grow, and numerous roots are developed further up on the stem to take its place. These adventitious roots are characteristic of monocotyledons, or plants with one seed leaf, to which the Gramineæ belong, although they are not confined to this class. In Fig. I., *r*, these adventitious roots are seen; the primary root is also here, but even in this early stage is barely to be distinguished from its adventitious neighbours. Each fibril is furnished with a root cap, and at some distance behind is clothed with absorbent root-hairs.

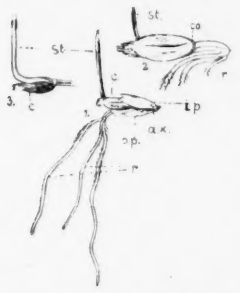

Fig. I.—Germinating Oat.

1. Showing paleæ which remain attached to fruit; *i.p.*, inner palea; *o.p.*, outer palea; *c.*, caryopsis; *r.*, adventitious rootlets; *a. x'.*, portion of axis of spikelet; *st.*, stem.

2. *Ibid*, with paleæ stripped off. The origin of the rootlets is clearly seen. *co.*, coleorhiza; *st.*, stem.

3. Other side of 2, showing origin of stem; *st.*, stem.

At the points where the subterranean runners, or stolons, send off roots aërial stems also arise. These places are called nodes, and the portions between internodes. Nodes and internodes alternate in the same manner in the aërial stem from whose nodes leaves only are normally developed. Excepting at the nodes the aërial stems are hollow, and there they are characteristically jointed. The nodal septa, or divisions, are formed by the growing together of the vascular bundles which, as continuation of the veins of the leaves, run down the stem parallel to one another and to the periphery. The sedges (natural order Cyperaceæ) are nearly related to the grasses, and some of them are very similar in appearance to their allies. The tubular stem of the grasses and the solid stem of the sedges is, however, a point of distinction easily noted. It is true that in hot regions a few grasses, like the sugar-cane (*Saccharum*), have solid stems, and that the stolons of our British grasses are not fistular, but a hollow aërial stem is characteristic of all British species.

Grass stems are always very slender. A remarkable instance of this is found in the bamboo, which may grow to a height of fifty feet, although the stem is only five inches in diameter. The hollow nature of the structure and the development of its strengthening tissue towards the circumference render it able to resist effectively the strains to which it is subjected. The result of the non-development of the normal strengthening tissue, or sclerenchyma (σκληρος, hard, and ενχυμα, a tube), is sometimes seen in a field of closely sown wheat, especially if the crop be a heavy one and the season wet. The lower portion of the stem is so shaded that assimilation is almost quite prevented, and in consequence that portion is deficient in sclerenchyma, resulting in the "laying" of the wheat. A large amount of silica is present in some grasses, and this was considered to be the strengthening material. "The thoughtless assumption that the rigidity of the haulms of cereals is essentially promoted by the silica which they contain impelled agriculturists, thirty years ago, to manure their wheat fields with costly preparations of silica, hoping thereby to prevent the laying of the wheat."* Large deposits of silica occupy the internodes of the bamboo. This substance, known as tabascheer or tabaxir, possesses peculiar optical properties.

The leaves are arranged on the stem in two rows. They have no petiole or leaf-stalk, but the leaf-sheath, which is split down one side, is well developed, and completely embraces the stem. Where it joins the blade or lamina, there is a tongue or ligule, generally of a scarious nature. The veins of the leaves, as in monocotyledons generally, run parallel to one another.

The flowers are arranged indefinitely on their axis, the older flowers being below, the younger above. One flower, or two or more arranged closely together on one stem, form a spikelet or little spike,† and there are all gradations from a spike of one-flowered spikelets, as in the Timothy

Fig. V.—Paniele of *Aira cæspitosa*.

* Sachs, *Physiology of Plants*, page 289.

† A spike is an indefinite or racemose form of inflorescence, in which the main axis is elongated and the individual flowers sessile. A raceme only differs from it in having stalked flowers.

grass (*Phleum pratense*), to a compound loose raceme or panicle of many-flowered spikes, as in *Aira cæspitosa*, *Arena* (the oat), and *Holcus lanatus* (the Yorkshire fog). Fig. V. represents the panicle of *Aira cæspitosa*, Fig. IV. a spikelet of *Holcus lanatus*, and Fig. III. that of the perennial rye-grass (*Lolium perenne*), whose inflorescence is a spike of many-flowered spikelets.

We will now examine the two spikelets figured. At Fig. IV., *st* is the short stalk of the spikelet, by which it is fastened to a lateral branch of the main axis of the inflorescence. It bears first two leaves (*g. g.*), called the glumes; in the young condition these enclose the flowers borne on its upper portion. One of them is furnished with an arista or awn (*a*). There are two flowers in this spikelet. Each arises in the axil of a leaf called the outer palea (*o. p.*), which is morphologically a bract, as the definition of a bract is "a leaf in the axil of which a single flower arises." In dicotyledons — for instance, in Leguminosæ —two small greenish bodies may sometimes be observed, situated laterally on the peduncle or flower stalk, and in monocotyledons one placed opposite the bract, their number thus coinciding with that of the cotyledons. These structures are called prophylls or bracteoles. What is commonly called the inner palea (*i. p.*) of the graminaceous flower is thus a prophyll. The outer palea often bears an awn. In *Holcus lanatus* the outer palea* of the upper flower only is furnished with this appendage, and it arises from the middle of the back.

The structure of the flower is peculiar, and departs much from the typical monocotyledonous type. In the Liliaceæ (lily order), which may be selected as the typical form of this class, there are five whorls of floral organs—the perianth (περι, around, and ανθος, a flower) or floral envelope, two whorls of three members each; the androecium (ανηρ, a man, and οικος, a house), two whorls of three stamens each; and the gynœcium (γυνη, a woman) of three carpels united together. The members of the successive whorls alternate with one another, and the odd sepal is anterior, that is, it is situated next the observer who holds the flower between himself and the axis on which it arises. This arrangement is seen in Fig. II. 1. In the Liliaceæ there is a conspicuous perianth denoting that the flowers of that order are indebted to insects for performing the work of cross-fertilization. The flowers of the Gramineæ are wind pollinated, or anemophilous (ἀνεμος, the wind, and φιλεω, to love), and as is usual in such cases the perianth is inconspicuous, being here represented by insignificant, generally microscopical, scales, usually two in number, termed lodicules (Fig. II., *l*). These swell and push the bract outwards when the stamens and stigmas are ripe, and thus expose the organs of reproduction for the purpose of pollination. The two lodicules alternate in position with the outer palea; if a third one is present it stands opposite the inner palea. The stamens (Figs. III. and IV. *a n*) are generally three in number. Their very fine filaments appear to be fastened to the back of the anthers—versatile condition—owing to the downward growth of the anther lobes. The odd stamen is anterior; it is thus the inner whorl that is undeveloped. The two carpels (Figs. II. and IV. *c*) are united together or syncarpous (συν, and καρπος, a fruit). The two large feathery stigmas (Fig. III., *st*) are admirably suited to intercept the pollen grains wafted thither by the wind.

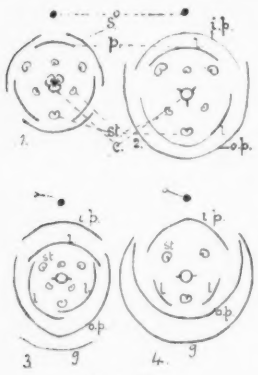

Fig. II.

1. Floral diagram of the Liliaceæ; *o.*, floral axis; *s.*, sepals; *p.*, petals; *st*, stamens; *c.*, carpels.

2. Ditto of Bamboo (*Bambusa arundinacea*); *l.*, lodicules; *o. p.*, outer palea; *i. p.*, inner palea.

3. Ditto of Rice (*Oryza*); *g.*, glume.

4. Ditto of Oat (*Arena sativa*).

A nearer approach to the structure of the typical monocotyledonous flower is made by that of the bamboo (Fig. II. 2), which possesses six stamens and carpels, but only three instead of six perianth segments. Six stamens are also present in some half dozen other genera, while in *Microlæna*, *Tetrarrhena*, and *Anomochlea* there are four. But two are found in *Anthoxanthum*—*Anthoxanthum odoratum* is the sweet-scented vernal grass—and one in Uniola and Monandraia (Fig. IIa., 5). These are thus more anomalous in form than Lolium and Holcus. Holcus, however, presents the anomaly of having hermaphrodite and male flowers on the same plant, a condition known as andro-monœcious (μονος, one)—see Fig. IV., where the lower flower of the spikelet is hermaphrodite, the upper male. Andromonœcism is also found in some members of the Fescue tribe. The rice (*Oryza*) has six stamens like the bamboo, but has only two carpels. In the maize (*Zea Mais*) the flowers are male or female, and both kinds are found forming female spikes and a male panicle on the one plant. The maize is thus monœcious. A curious change occurs in the female flower of Job's tears (*Coix lachryma*), a native of tropical Asia. The outer glume becomes bony and completely invests the mature spikelet, making the whole appear like a seed with a very hard case. The diagram of this anomalous form (Fig. IIa., 6) shows the position of the parts.

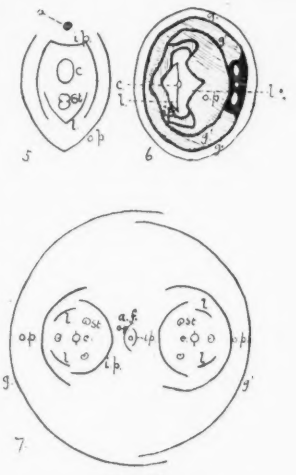

Fig. IIa.

5. Floral diagram of *Monandraia glauca*.

6. Ditto of Job's Tears (*Coix lachryma*); *g*, outer glume, forming bony involucre investing the spikelet; *g'*, inner glume. (After Le Maout and Decaisne).

7. Spikelet of Oat; *a. f.*, aborted flower.

Fig. IIa., 7, is a diagram of a spikelet of the oat (*Arena*). The fruit is usually composed of two carpels, which enclose a single cavity containing a single ovule. It is generally the case that the seed or seeds are free from the wall of the ovary, but in grasses they are united together, and the resultant dry fruit, which does not open to allow its seeds to escape, is called a *caryopsis*. A longitudinal section of the maize fruit taken through the centre of the embryo exhibits the parts shown in Fig. IV. The pericarp (περι, around, and καρπος, a fruit) is formed from the united nucellus or wall of the ovule, and true pericarp or ovarian wall. The embryo occupies a very small portion of the cavity of the ovule, the remainder of the embryo

* The outer palea has also been termed the flowering glume, and the glumes the outer glumes. As a rule, the latter organs do not bear flowers in their axils.

sac being filled with nourishing matter called endosperm (ἐνδο, within, and σπερμα, a seed). Only a small part of

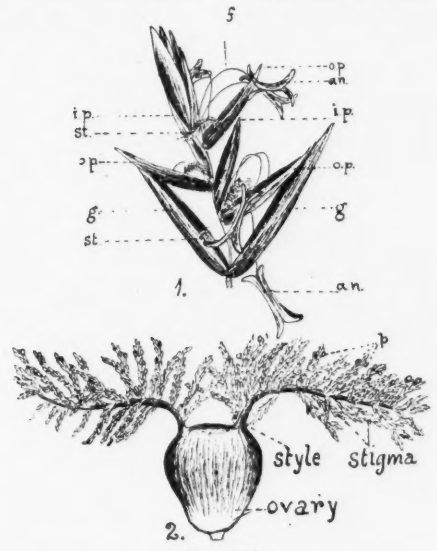

Fig. III.

1. Spikelet of the Perennial Rye Grass (*Lolium perenne*); *f.*, filament; *an.*, anthers; *i. p.*, inner palea; *o. p.*, outer palea; *st.*, stigma; *g.*, glume.
2. Gynœcium of *Lolium perenne*.

the endosperm is shown in the figure, but enough is portrayed to show that it is differentiated into a yellowish and a whitish portion, the former rich in nitrogen (albuminoid), the latter starchy. The embryo is composed of two main portions, the radicle and plumule. The lower portion of the radicle is the primary root, and the upper part the hypocotyl (ὑπο, under) or portion below the cotyledon or seed leaf. The plumule is the epicotyledonary part covered by its young leaves. This will afterwards form the greater part of the stem of the plant. The growing apex of the plumule is seen at *ap*. The embryo lies outside the endosperm, but it is attached to it by a large shield-shaped development of the hypocotyl, which at the same time nearly encloses the embryo itself. The epithelium of the scutellum, as this extraordinary structure is called, secretes ferments which digest the nourishing matter of the endosperm, changing the insoluble starch into soluble sugar, and the insoluble nitrogenous matters into soluble forms. These chemical changes commence when the mature fruit is placed in a moist medium, supplied with oxygen, and kept at a proper temperature. The soluble nutritive substances are transferred to the embryo plant by way of the scutellum, which thus acts like a placenta, and the seed commences to germinate. The radicle is pushed out of the fruit and the primary root penetrates the soil. Then the plumule makes its appearance, but the scutellum still remains attached to the endosperm, so that the seedling is still parasitic on the seed. Gradually the endosperm is used up, and by this time the roots have penetrated the soil and chlorophyll bearing leaves have so developed as to enable carbon assimilation to proceed sufficient for the needs of the seedling. The young plant can then elaborate food material for itself.

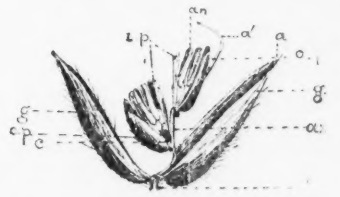

Fig. IV.—Spikelet of Yorkshire Fog (*Holcus lanatus*); *ax.*, stalk of spikelet; *ax'.*, axis of flowers of spikelet; *g.*, glume; *o. p.*, outer palea; *i. p.*, inner palea; *a.*, awn of glume; *a'.*, awn of outer palea of upper flower; *an.*, anthers; *c.*, gynœcium.

The delicate apex of the root is protected by a cap of cells (*r. c*). The roots of grasses are enclosed in a sheath (*r. s*), which they break through in germination, and which then surrounds their base like a collar. It is called the *coleorhiza* (ῥιζα, a root). This structure occurs in some other monocotyledons and in a few dicotyledons. Adventitious roots are developed from the hypocotyl, and rupture the enclosing tissue, which remains like a collar, as in the case of the root-sheath. The primary root of the maize remains for a comparatively long period much larger than the secondary roots. In the oats, at a very early period of germination, it is a matter of some difficulty to determine the primary root (see Fig. I.). But in any case the primary root soon dies away, and the development of a large number of adventitious roots produces the fibrous roots common to the majority of monocotyledonous plants.

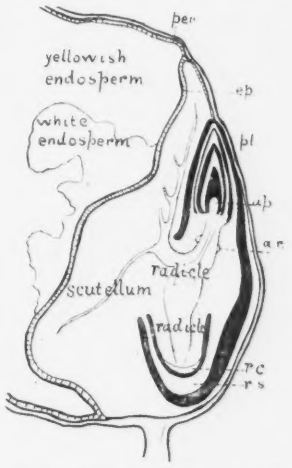

Fig. VI.—Longitudinal section of Maize fruit, showing only a portion of the endosperm. *ep.*, epithelium of scutellum; *per.*, pericarp, composed of wall of ovule (nucellus) and ovarian wall united together; *pl.*, plumule; *ap.*, apex of stem; *a. r.*, adventitious root; *r. c.*, root cap; *r. s.*, root sheath.

The products derived from grasses are extremely various, and the plants furnishing these are very numerous. The most familiar examples of economical products are derived from the cereals—wheat, barley and oats. The origin of the two former is shrouded in mystery. They have both been cultivated in prehistoric times, and consequently to establish the home of the wild species in each case is a matter of great difficulty. "Very ancient Egyptian monuments, older than the invasion of the Shepherds, and the Hebrew scriptures show this (the wheat) cultivation already established, and when the Egyptians or Greeks speak of its origin they attribute it to mythical personages, Isis, Ceres, Triptolemus." * De Candolle also states that the Chinese, who cultivated wheat in B.C. 2700, considered it a gift direct from heaven, and that it is one of the five species of seeds annually sown in the ceremony of sowing five kinds of seeds—rice (*Oryza*), sorghum (a kind of millet), *Setario italica* (another kind of millet), and soy being the others. He considers the Euphrates valley to be the probable home of the species. The account given by the same authority of the supposed mummy-wheat is worth quoting. "No grain taken from an ancient Egyptian sarcophagus and sown by horticulturists has ever been known to germinate. It is not that the thing is impossible, for grains are all the better preserved that they are protected from the air and from variations of temperature or humidity, and certainly those conditions are fulfilled by Egyptian monuments; but, as a matter of

* De Candolle, *Origin of Cultivated Plants*, page 354.

fact, the attempts at raising wheat from these ancient seeds have not been successful. The experiment which has been most talked of is that of the Count of Sternberg, at Prague. He had received the grains from a trustworthy traveller, who assured him they were taken from a sarcophagus. Two of these seeds germinated, it is said; but I have ascertained that in Germany well-informed persons believe there is some imposture, either on the part of the Arabs, who sometimes slip modern seeds into the tombs (even maize, an American plant), or on that of the *employés* of the Count of Sternberg. The grain known in commerce as mummy-wheat has never had any proof of antiquity of origin." *

Barley (*Hordeum vulgare*) may probably belong to Mesopotamia, but there exists a doubt with regard to the matter. De Candolle believes that all species of oats (*Avena*) cultivated are derived from a prehistoric form, and a native of eastern temperate Europe and Tartary. The common species is *Avena sativa*, of which, as in the cases of wheat and barley, there are numerous varieties in cultivation.

The most wonderful of all the grasses is the bamboo (*Bambusa arundinacea*).† It may attain a height of even eighty feet, and the rapidity with which it grows may fairly be said to be extraordinary—sometimes two to two and a half feet a day. The young shoots are eaten by the Chinese as we eat asparagus. Water buckets and water bottles are made from its stem if the joints be large enough. The stem is also used as timber in a variety of ways; and finer kinds of paper are manufactured by the Chinese from the inner portion of it. It can also be used for masts, and for agricultural and domestic implements, and we are familiar with it in the form of umbrella handles and walking sticks. A writer in the *Gardeners' Chronicle* of July 25th, 1891, records a remarkable phenomenon he witnessed in the province of Malabar—the seeding of the bamboo. "Hundreds of square miles were thickly covered with graceful clumps of this plant, many specimens being from sixty to seventy feet high. The leaves left the lateral branches and at the same time the inflorescence made its appearance, changing the aspect of the country as if by magic. No one was prepared for such an eventuality, and the English planters in the district were struck with something akin to dread when the fact dawned upon them that, in the course of a very brief period, not a living bamboo would be left in the forest. A few there were who refused to believe that the culms would perish after ripening their seeds, and were only persuaded by the actual realization of the fact." In the middle of May the seed came to maturity, and then there only remained the dead stems where once was a leafy forest of green waving plumes.

Of *Saccharum officinarum*, the sugar-cane, another interesting member of the Gramineæ, want of space prevents us speaking.

THE REV. JOHN MICHELL, ASTRONOMER AND GEOLOGIST.

By John Richard Sutton, B.A.Cantab.

THE world of to-day has almost forgotten John Michell, but no more bold and original thinker ever devoted himself to the study of the earth or the heavens. He lived at a time immediately preceding the advent of William Smith and Sir William Herschel, and his greatness has been kept in the background by the honour which the world has awarded to his successors. His name is mentioned occasionally, in authors' footnotes chiefly; and some later writers—notably Sir Charles Lyell, Prof. Grant and Arago—give him a share of commendation, though with little enthusiasm.‡ Of all the great cyclopædias, the *Encyclopædia Britannica* alone deigns to mention him at all, and even then incorrectly, though every one of them gives whole pages to lesser men. To a certain extent this is due to the fact that Michell's speculations were a full century before his age.

Michell was entered at Queen's College, Cambridge, and graduated fourth wrangler in 1748§—a position scarcely worthy of his powers.

In many ways there is something incongruous in associating Michell and his severe mental characteristics with the ivy-clad cloisters of Queen's and their memories of Erasmus and Ridley. Newton at Trinity, Wordsworth at St. John's, Young at Emmanuel, Cromwell at Sidney Sussex, where altar-candles are still waging an unequal battle with the persistent spirit of the old Puritanism—these are all natural figures in keeping with their college surroundings. But if college walls breathe inspiration at all, then assuredly neither Art nor Nature ever intended Queen's to be the abode of the hard and realistic sciences.

All the same, Michell flourished at Queen's. He took holy orders, and was elected to a fellowship immediately after taking his degree. He was elected Prælector Hebraicus and Prælector Arithmeticus in 1751, Censor Theologicus and Examinator in 1752, Prælector Geometricus in 1753, Censor Theologicus in 1754, Prælector Græcus in 1755. From 1756 to 1759 he was Senior Bursar (Thesaurius Superior). In 1762 he was again Prælector Hebraicus.‖

In 1750, only two years after taking his degree, he published his first work, an interesting little book of eighty-one pages, entitled "A Treatise of Artificial Magnets, in which is shown an easy and expeditious *method* of making them superior to the best *natural* ones, and of changing or converting their *poles*"—a most charming little volume, evidently the production of a mind above the common. Some of the experiments he describes indicate almost an instinct for tracing the workings of Nature, and are none the less interesting because of his most fascinating command of language. In this respect, indeed, Sir John Herschel was scarcely his superior.

In 1760, Michell was elected to the fellowship of the Royal Society, and in this year he published a paper on the cause and phenomena of earthquakes. The paper, which is very lengthy, is what such a scientific discussion should be; first a collection of facts, and then intelligent reasoning upon them. Certainly Michell shows, at this early stage of his career, a power of reasoning upon his facts which, up to that time, had been unknown in the history of geology. No available sources of information were neglected. The spirit of Epicurus was in the man tenfold. His words, to modern readers, convey the idea of prophecy fulfilled, rather than of deductions likely to have much weight at the time. "Some of his observations anticipate in so remarkable a manner the theories established forty years afterwards, that his writings would probably have formed an era in the science, if his

* De Candolle, *Origin of Cultivated Plants*, page 362.

† There are other species, as *Bambusa verticillata*, etc.

‡ It is due, however, to Mr. Ranyard to acknowledge that in the *Old and New Astronomy* he has spoken of Michell's work in a most appreciative way.

§ Also M.A. in 1752; B.D. in 1761.

‖ The author is indebted to the Rev. J. H. Gray, M.A., Dean of Queen's College, for these facts.

researches had been uninterrupted," as an eminent man of science admitted.* The whole paper should be studied.

When the Woodwardian Professorship of Geology at Cambridge became vacant in 1762 it was rightly felt that Michell was the man for the post, "and there is every reason to believe that had he retained the office for any length of time he would have done much to rescue the title of Woodwardian Professor, as well as the museum, from the species of contempt in which both were long allowed to remain. Unfortunately, however, after two short years he took leave of Cambridge and the Woodwardian possessions, attracted by the superior charms of a wife and a living."†

Of these two crowning blessings the wife came first, the living not until the end of another three years; for after 1764 Michell is not heard of at Queen's, and the natural inference would be that he vacated his fellowship in order to marry. It was obviously no flighty marriage, since he was pretty nearly forty years of age at the time and his wife seven years younger. Fortunately, marriage and his living did not withdraw him from scientific work, for the period between 1764 and 1767 (the latter year dating the commencement of his residence at Thornhill) was one of the most fruitful periods of his life.

In 1765 he was one of a committee of six ("three gentlemen skilled in mechanics and three watchmakers"), appointed by the Commissioners of Longitude, to examine the chronometers then recently invented by John Harrison. An Act of Parliament had been passed in 1714, offering for an exact chronometer a reward of "£10,000 if it determine the longitude to one degree of a great circle, or sixty geographical miles; £15,000 if it determine the same to two-thirds that distance; and £20,000 if it determine it to one-half that distance"; adding, "that one-half of such reward shall be due and paid when the said Commissioners, or the major part of them, do agree that any such method extends to the security of ships within eighty geographical miles from the shore, which are places of the greatest danger; and the other half when a ship by the appointment of the said Commissioners, or the major part of them, shall sail over the ocean from Great Britain to any such port in the West Indies as those Commissioners, or the major part of them, shall choose or nominate for the experiment, without losing her longitude for the limits above mentioned." After many delays and much bickering from Harrison, who, as a practical man, had strong objections to "men of theory," the committee was made up of "the Rev. John Michell, late Woodward Professor of Geology, the Rev. Will Ludlam, Fellow of St. John's College, Camb., John Bird, mathematical instrument maker, with Thomas Mudge, William Matthews, and Larcum Kendal."‡

A better man than Michell could not have been selected for this examination, and it is pretty clear that the committee as a whole did their duty very well. Harrison's chronometer was found on trial to keep the time so accurately as to allow the longitude to be ascertained to within one quarter of a degree, a perfection not anticipated by Parliament. The clever but slightly over-reaching watchmaker at once thought himself entitled to the prize, without disclosing the principle of his instrument, a consideration which would have completely nullified the purpose of the Act. Then the bickerings commenced afresh—Michell, and those with him, rightly declining to recommend the award unless the whole plan and construction of the chronometer were forthcoming; Harrison, whose temper was none the more docile for certain shabby treatment he had received from the Government, declining to divulge it, claiming that he had done all that the law required in *making* the instrument—and it was not until many words had been wasted on all hands that he could be persuaded to give way, to the great profit of navigation.§

Michell next appeared before the scientific public with two useful papers on the use of Hadley's quadrant for surveying and pilotage, and on a new method for measuring degrees of longitude, respectively, neither of which, however, need be explained here.

Finally, before settling down as a country parson, he published "An Inquiry into the probable Parallax and Magnitude of the Fixed Stars, from the quantity of light which they afford us, and the particular circumstances of their situation."¶ A finer paper has probably never been read before a scientific assembly—indeed, modern stellar astronomy has not got so very far beyond the verification of the conclusions it set forth. First, in a course of elegant arguments, Michell shows that the nearest fixed star is not very likely to be more remote than 220,000 times the radius of the earth's orbit. His procedure is simple, so simple indeed as to make one wonder why it was not thought of before. He compares the total light received from the sun with the average amount received from the first magnitude stars. To do this with absolute accuracy it would be necessary in the first place to obtain the actual amount of light received from the brightest stars by some such method as the use of, say, Prof. Pickering's photometer, or a wedge, and then to compare this with the light of the sun. But this would be a most laborious process, and not to be lightly undertaken; nor, in truth, were any such accurate means of light-measurement then known. Yet, even though the days of wedge and meridian photometers had not come, Michell could still show a very short and simple way of dealing with the question. Consider the planet Saturn when in opposition, and when both Saturn and the earth are at their respective mean distances. In this case, it takes but a rough observation to show that Saturn is not much brighter than the average first magnitude star. Take away the planet's ring and they will be nearly equal. Now, assuming that Saturn reflects *all* the sun's light which falls upon him—an assumption not strictly true, but near enough — and combining this with the consideration that the light received from a bright area varies inversely as the square of its distance, we have the following easy calculation:—The intrinsic brightness of Saturn is to the intrinsic brightness of the sun as the square of the distance of the sun's surface from its centre is to the square of Saturn's distance from that centre. Now, Saturn is about 2082 times further from the sun's centre than the sun's surface is; therefore the brightness of Saturn is to the brightness of the sun as unity is to 2082×2082. Again, the sun's diameter is about 105 times the diameter of Saturn; hence the sun's disc will be 105×105 times greater than Saturn's. And to

* Sir Charles Lyell: *Principles of Geology*, 9th ed., p. 41.

† From an historical sketch of the Woodwardian Museum in Smith's *Cambridge Portfolio*, by David Ansted, formerly Fellow of Jesus College. Sir David Brewster has observed that Michell held the Woodwardian Professorship for eight years, and Lyell repeats the statement. But Michell could not have held it for more than two years, for in 1765 he is spoken of as *late* Woodwardian Professor; nor would he have been allowed to hold that office and live at Thornhill at the same time. It is said that we owe to Michell many of the terms now in use among geologists, such as "Gault," "Coal-measures," &c. See also Woodward's *Geology of England and Wales*.

‡ Dodsley's *Annual Register* for 1765.

§ *Ibid.*

‖ *Phil Trans.* for 1765 and 1766.

¶ *Phil. Trans.*, Vol. LVII., p. 234.

compare the actual amount of light sent out by each, we have to multiply the numbers representing the areas by the numbers representing the intrinsic brightness. Making the calculation, we see that the light of the sun is to the light of Saturn as $2082 \times 2082 \times 105 \times 105$ is to unity. Now, the square root of the product just given is 218,210; which means that we should have to remove the sun to about 218,210 times its present distance before it would shine as a first magnitude star.

This, of course, is only an approximation, and only useful (but very useful, nevertheless) in framing first and elementary ideas as to the extent of the visible universe. Michell was careful to point out that although his method might be trusted to give trustworthy *general* data to start with, it would not be applicable to special cases; for, as he warns us with one of his touches of inspiration, most likely the stars are not by any means of an equal size, and the surface brightness may differ very greatly in different stars.

The paper then goes on to discuss methods for determining the actual sizes of the stars, and the difficulty—nay, the impossibility—of ever solving this great problem: "There seems to be little chance," says Michell, "of discovering with certainty the real size of any of the fixed stars, and we must consequently be content to deduce it from their parallax, if that should ever be found, and the quantity of light which they afford us compared with that of the sun. And in the meantime, till this parallax can be found, or something else may arise to furnish us with a more general analogy, we can only suppose them, 'at a medium,' to be equal in size to the sun, this being the best means which we have at present of forming some probable conjecture concerning the extent of the visible universe. That we may be the better enabled to do this, it seems to be an object worth the attention of astronomers to inquire into the exact quantity of light which each star affords us separately when compared with the sun; that instead of distributing them, as has hitherto been done, into a few ill-defined classes they may be ranked with precision, both according to their respective brightness and the exact degree of it."

Let us pause for a moment to see what modern astronomy has to say to Michell's results. Seventy years had to pass before Mr. Henderson and Sir Thomas Maclean could prove to the world from observations on the parallax of Alpha Centauri that Michell's theoretical estimate was not far from the truth. Moreover, the qualifications he had laid down as to the various sizes of the stars were also shown to be justified by the results of a comparison between the discoveries of Henderson and Maclean, and those of Bessel on the star 61 Cygni; for this latter star, although only about three times as far away, gives us scarcely a two-hundredth part of the light of Alpha Centauri, whereas if all the stars were equal in size and brightness it would send us nearly thirty times as much light as it does. If it should be urged that Michell's speculations were nothing but lucky guesses, it may be retorted that such luck comes only to genius. For the rest, it may be worth noting that Michell's dream of a star-catalogue, in which all the stars should be "ranked with precision," has only begun to be realized within the last twenty years or so.

The best part of Michell's extraordinary paper is his application of the theory of probabilities to the distribution of the stars. It has always been the custom to divide the stars into groups or constellations, for religious or historical purposes, or for convenience of reference. Generally speaking, these groups or constellations are well-defined: Orion, Ursa Major, and the Pleiades, for example. From the time of Copernicus this grouping had been regarded as accidental, and the constellations were supposed to result from the projection of a number of bright stars, differing greatly in distance, upon small portions of the imaginary sphere of the heavens. Michell then took up the parable, and applied the doctrines of probability to the question for the first time in the history of astronomy. Dealing with the brighter stars in the Pleiades, as being the most convenient for his purpose, he was able to show that the odds were 500,000 to one against any six physically unconnected stars scattered at random through space being found projected upon the sky so closely as these are.* Hence it is nearly certain that the Pleiades (and other similar groups, of course, by the same reasoning) are in reality physically, as well as optically, associated. Unfortunately, this grand result, so valuable in itself, and so full of promise to further research along the same lines, was practically neglected, and astronomers took over a century to rediscover that which Michell with good reason regarded as demonstrated. The observations of Bessel, Wolf, and Elkin shows that the greater number of stars in the Pleiades group have a common small proper motion, which seems to indicate that the whole group is a connected system; and the more recent application of photography has confirmed this theory by revealing the remarkable fact that a gigantic nebula envelopes the whole or the greater part of the whole group. An additional and strong argument was also advanced on the same side by the late Mr. Proctor, founded upon the small number of stars in the sky-space adjacent to the Pleiades, suggesting, so great is the contrast, a sea of darkness enclosing an island of light. Surely, if the greatness of Michell needed demonstration these facts ought to be enough. It may be added that the laws of probability apply in exactly the same way to cases where two, or ten, or any number of stars are seen close together on the sky, though the odds in favour of a physical connection in such instances are much reduced as the number of stars so situated is less.

The remainder of the paper is taken up with a discussion of the relations presented by particular circumstances of star-grouping, and the possible position of the sun amidst the stars. The results in the main are nothing less than what astronomers during the last twenty or thirty years have begun to see clearly. But singular injustice has been done to Michell in this as in all things else. Recent writers have either appropriated his work without acknowledgment, or have too generously handed it over to others. So the intimation that stars optically close are almost certain to be physically connected has been placed to the credit of Sir William Herschel, who simply determined the orbital *motions* of some of these close stars, and describes himself as surprised at the discovery of their connection. In speaking of the discovery, he says that he went out like Saul to seek his father's asses, and found a kingdom—the dominion of gravitation extending to the stars. It had been known for a very long time that many of the stars which appear single to the naked eye are double or triple when seen in the telescope; and Michell's paper was a satisfactory proof of the significance of the phenomena. The great telescopic powers which Herschel used enabled him to split up a great many more stars which were not previously known to be double, but so far was he from grasping the physical explanation of his facts that, in his first catalogue (published in 1782), he sees no other reason for a change of position in the components of a double star than "by admitting a proper motion in either

* Sir John Herschel points out that when the stars of the southern hemisphere are included in the investigation these figures have to be considerably reduced. The correctness of the argument is unaltered, however.

one or the other of the stars, or in our solar system." But in 1784 Michell pointed out that the great and increasing number of known double stars could leave no doubt in any properly-balanced mathematical mind (even if his former paper, in spite of its lesser basis of knowledge, were not sufficiently convincing) "that by far the greater part, if not all of them, are systems of stars so near each other as probably to be liable to be affected sensibly by their mutual gravitation; and it is therefore not unlikely that the periods of the revolutions of some of these about their principals (the smaller ones being on this hypothesis considered as satellites of the others) may some time or other be discovered."* Very little notice seems to have been taken of this confident prediction. Twenty years passed, and the scientific world exclaimed, *How wonderful!* when Herschel's observations forced him to acknowledge that many of these double stars were really binary systems, whose components revolve about each other in the same way as the earth and moon.

All this is in Michell's one paper of 1767†. Had he never after put pen to paper, nor engaged in any further scientific pursuits, enough had been accomplished to place him in the front rank of English philosophers. Not only did he invent his own method of procedure in his researches, but as its result he confidently announced the solution of problems which men of that day almost thought it presumptuous to speak of. The theory of probabilities not being then appreciated and relied on as now, there seemed more of shadow than of substance in his speculations. More attention would have been accorded him by his contemporaries, perhaps, had he, like Herschel, done something sensational. Herschel attracted little attention until his discovery of Uranus burst upon the world. But Michell did nothing to take the world by storm.

(*To be continued.*)

* *Phil. Trans.*

† Mr. Sutton has not referred to what seems to me to be the most striking and remarkable suggestion in this paper, in which Michell anticipates Pickering's and Monck's methods of comparing the brightness or density of double stars. It is contained in a note on page 238 of his paper, and most concisely points out how, if we know the period and brightness of a double star, we may neglect its parallax, and directly compare its brightness and mass with that of the sun. Prof. E. C. Pickering, in 1880, showed how, in dealing with a binary star system, the parallax might be neglected, and the density or brightness of the double star might be compared directly with that of the sun. Mr. Monck, in the *Observatory* of 1887, showed how the brightness or density of one binary system might be compared with that of another binary system. Neither Pickering nor Monck seem to have known of Michell's note which anticipates their whole line of reasoning. It runs as follows:—

"If, however, it should hereafter be found that any of the stars have others revolving about them (for no satellites shining by a borrowed light could possibly be visible), we should then have the means of discovering the proportion between the light of the sun and the light of those stars, relatively to their respective quantities of matter, for in this case the times of the revolutions and the greatest apparent elongations of those stars that revolved about the others as satellites being known, the relation between the apparent diameters and the densities of the central stars would be given, whatever was their distance from us, and the actual quantity of matter which they contained would be known whenever their distance was known, being greater or less in proportion to the cube of that distance. Hence, supposing them to be of the same density with the sun, the proportion of the brightness of their surfaces, compared with that of the sun, would be known from the comparison of the whole of the light which we receive from them with that which we receive from the sun; but if they should happen to be either of greater or less density than the sun, the whole of their light not being affected by these suppositions, their surfaces would, indeed, be more or less luminous, accordingly as they were upon this account less or greater, but the quantity of light corresponding to the same quantity of matter would still remain the same.

"The apparent distances at which satellites would revolve about any stars would be equal to the semi-annual parallaxes of those stars, seen from planets, revolving about the sun in the same periodical times with themselves, supposing the parallaxes to be such as they would be if the stars were of the same size and density with the sun."

At the date of Michell's paper (1767) less than seventy close double stars were known, and none were known to be moving about one another, but in this paper, as well as in a second paper published in the *Phil. Trans.* for 1784, he speaks with confidence as to the physical connection between such closely situated pairs.—A. C. Ranyard.

WHAT IS A NEBULA?

By A. C. Ranyard.

THERE can be little doubt that the nebulæ are, as a general rule, very transparent, for it cannot reasonably be doubted that by far the greater number of the stars which appear to us surrounded by nebulous matter are really involved in the brighter and central parts of the nebulæ which appear to surround them, and are not merely seen by chance projected on a nebulous background. A very elementary application of the doctrine of chances will show the enormous improbability of the hundreds of nebulous stars known, all being seen projected (generally pretty centrally) upon a nebulous background. In the case of the great nebula in Orion, and of the Pleiades nebula, as well as in some of the smaller nebulæ, we actually see nebulous structures which appear to spring from stars or groups of stars like the trapezium in the Orion nebula; the nebulous structures grow gradually fainter as the distance from the star from which they appear to spring increases, and in some instances the nebulous structures branch or divide in a direction away from the star in a manner which leaves no room for doubting that the seat of origin of the structure (that is, the place from which it was belched forth) must have been within the star.

We may therefore feel practically certain that we receive the light from many stars after its passage through many thousands of millions of miles of nebulous matter. We know how the light of our own sun is dimmed at sunset and sunrise by its passage through a few hundred miles of our atmosphere, so that the eye can easily gaze on the sun's disc; and a photograph which at midday can be obtained in a fraction of a second, takes at sunset or sunrise many seconds, or even some minutes, to give it a suitable exposure. The almost perfect transparency of this nebulous matter will be best realized by the student of physics who knows that if half the light were absorbed in its passage through ten million miles of nebula, only a quarter of the light radiated by the star would get through twenty million miles of similar nebulous matter, and only an eighth part through thirty million miles, only a sixteenth through forty million miles, and so on; for each succeeding ten million miles of similar nebulous haze would halve the light which had succeeded in getting through the nebulous veil between it and the source of light. Thus at a distance of 200 million miles from the nebulous star the star's light would be reduced to about one millionth of the emitted light; at a distance of 400 million miles it would be reduced to a millionth of a millionth of its original brightness; and after passing through 1000 million miles its light would have been reduced 75 magnitudes in the stellar scale of brightness.

Such vast distances are triflingly small when measured by the scale of architecture on which nebulæ are built. A nebula a thousand million miles in diameter at the distance of α *Centauri* (our nearest neighbour amongst the stars) would only appear to have a diameter of 8·12″ seconds of arc, while the nebula in Orion has a diameter of more than half a degree, and the great nebulous and stellar structure shown in our plate has a diameter of more than 20°.

There is another class of reasoning which enables us in a vague and rough way to fix a superior limit for the density of these nebulous masses. If we suppose the Orion nebula to be a uniform sphere of only a third of a degree (20′) in diameter, with an average density of only one millionth of our atmospheric air at the sea level, the mass of the nebula would be such that at the distance of α *Centauri*, assuming gravity to act across interstellar

space,* it would be capable of giving our sun a velocity of 255·2 miles per second if it fell towards the nebula from an infinite distance, and the velocity of our sun in a circular orbit about such a nebula, situated at a distance equal to the distance of *α Centauri*, would be 180·4 miles per second. If the nebula contained such a vast mass of attracting matter we should expect to see many stars in the neighbourhood of the nebula moving across the line of sight with very large proper motions, for a velocity of 100 miles a second across the line of sight at the distance of *α Centauri* would give an annual proper motion of 25·5″, that is a proper motion more than three times as great as that of 1830 Groombridge, which is the swiftest moving star which has at present been discovered.

But instead of finding the stars in the immediate neighbourhood of the Orion nebula exhibiting large proper motions, we find the stars in this region of the heavens, which appear to be associated with the Orion nebula, show hardly any detectable proper motion, and the same remark applies to all the stars connected with the stream of nebulæ which appear to link up the great Orion nebula with the Milky Way. The stars of the Pleiades group, which also appears to be connected with the Milky Way and to be surrounded by a very extensive nebula (see Knowledge for May, 1891), also exhibit only small annual proper motions, and the same remark applies to the nebulous star *α Cygni*, which appears to be associated with the nebulosity of the Milky Way (see Knowledge for October, 1891).

If, instead of assuming the distance of the Orion nebula to be equal to the distance of *α Centauri*, we had assumed its distance to be double as great, the velocities referred to above, of our sun in a parabolic orbit or in a circular orbit about the nebula would need to be doubled. For at twice the distance a nebula subtending the same angular diameter would occupy eight times the volume, and, the density remaining the same, its mass would be eight times as great, and the periodic time in a circular orbit about such a nebula at double distance would be unchanged, therefore the velocity in the larger orbit would be doubled.

That the periodic time is independent of the distance of the nebula will be evident when it is remembered that the square of the periodic time, in any orbit, is inversely proportional to the attracting mass. Therefore, if the central attracting mass be multiplied by eight, the size of the orbit remaining unaltered, the periodic time will be reduced in the proportion of 1 to $\frac{1}{2\sqrt{2}}$. On the other hand, if it is evident from Kepler's law connecting the squares of the times with the cubes of the distances, that if the attracting mass remains unaltered, and the size of the orbit be doubled, the periodic time will be increased in the ratio of 1 to $2\sqrt{2}$; therefore, if the size of the orbit be doubled, and the attracting mass be multiplied by eight, the periodic time will remain unaltered,† and the velocity in the orbit will be doubled if we double the distance of the nebula.

If we assume the distance of the Orion nebula to be equal to the distance of *α Centauri*, that is, taking the parallax of *α Centauri* as 0·75″, or that it is situated at a distance of 274,900 times the earth's distance from the sun, and that the mean density of the nebulous matter is one hundred millionth of the density of atmospheric air at the sea level—that is, that the density of the nebula is about $\frac{1}{122,160,000,000}$th of the density of the sun—for water is about 846 times as heavy as an equal volume of air at the sea level, and the density of the sun is about 1·444 as compared with water; the mass of the nebula, supposing it to be spherical and to have an angular diameter of 20′ would be 330,200 times the mass of the sun, and the periodic time in a circular orbit about such a body at a distance equal to the distance of *α Centauri* from the sun would be 281,450 years, which corresponds to a velocity of a little more than 18 miles a second. If we suppose the nebula to be at double the distance of *α Centauri* the velocity in a circular orbit would be 36 miles a second, and so on, the velocity increasing directly as the distance of the nebula is increased.

We may probably feel quite sure, from the small observed proper motions in the neighbourhood of the Orion nebula, that its average density does not exceed one ten thousand millionth of the density of atmospheric air at the sea level. This would about correspond to the mean density of the solar nebulous mass, supposing it to have been spherical when its radius was a little more than 107 astronomical units, or when the sun occupied a sphere with a radius of a little more than 3½ times the distance of Neptune.

In examining the forms of nebulæ we find comparatively few oblate spheroids, such as the hypothesis of La Place assumes. There are many apparently spherical masses, a few spirals and rings, and a great many nebulous masses of irregular form. If the stars we see are of very different ages, and the nebular stage of condensation occupies, as has hitherto been supposed, a very lengthy period compared with the stellar stage, we should expect to see a far greater number of nebular masses than of fully-formed stars, but the number of brightly shining stellar points greatly exceeds the number of nebular masses hitherto discovered. Possibly we are mistaken in supposing that the faintly shining nebular masses we observe afford ocular evidence of the truth of La Place's bold hypothesis. The nebulæ we see have, it seems to me, a greater analogy with the solar corona than with the fiery condensing mists conceived of by La Place; they are very generally associated with stars, and in some cases the nebulous structure clearly indicates that the nebulous matter has issued from the star, and sometimes from a starless region. The forms of nebulæ are certainly in general inconsistent with the theory that stars are condensing from nebulæ.

The dark tree-like structure which extends diagonally from the right lower corner of our plate up towards the left hand upper corner clearly tells the story of matter projected into a resisting medium, and not of nebulous matter slowly condensing under the influence of steady rotation. If the reader will take the trouble to compare two copies of the plate—one a dark print and the other a print in which the nebulosity is whiter—he will recognize several smaller tree-like forms extending laterally on either hand from the main trunk of the dark structure, as well as several streams of stars evidently springing from the dark region.

* In view of what we know of the motions of binary stars about one another, the philosopher who has serious doubts whether the action of gravity extends across interstellar space must have a very highly-developed organ of philosophic doubt. He must be willing to assume that while gravity extends apparently, according to law, across the space which separates the sun and Neptune, and across the probably still wider spaces which separate many of the binary stars, its action ceases or is interfered with in passing across the interstellar spaces which separate the systems within which gravity reigns.

† The above reasoning only holds when the mass of the sun may be neglected as compared with the mass of the nebula.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

THE ATMOSPHERES OF CELESTIAL BODIES.

To the Editor of Knowledge.

Sir,—In discussing the atmospheres of the moon or of Mars, I think we rely too much on the analogy of our own

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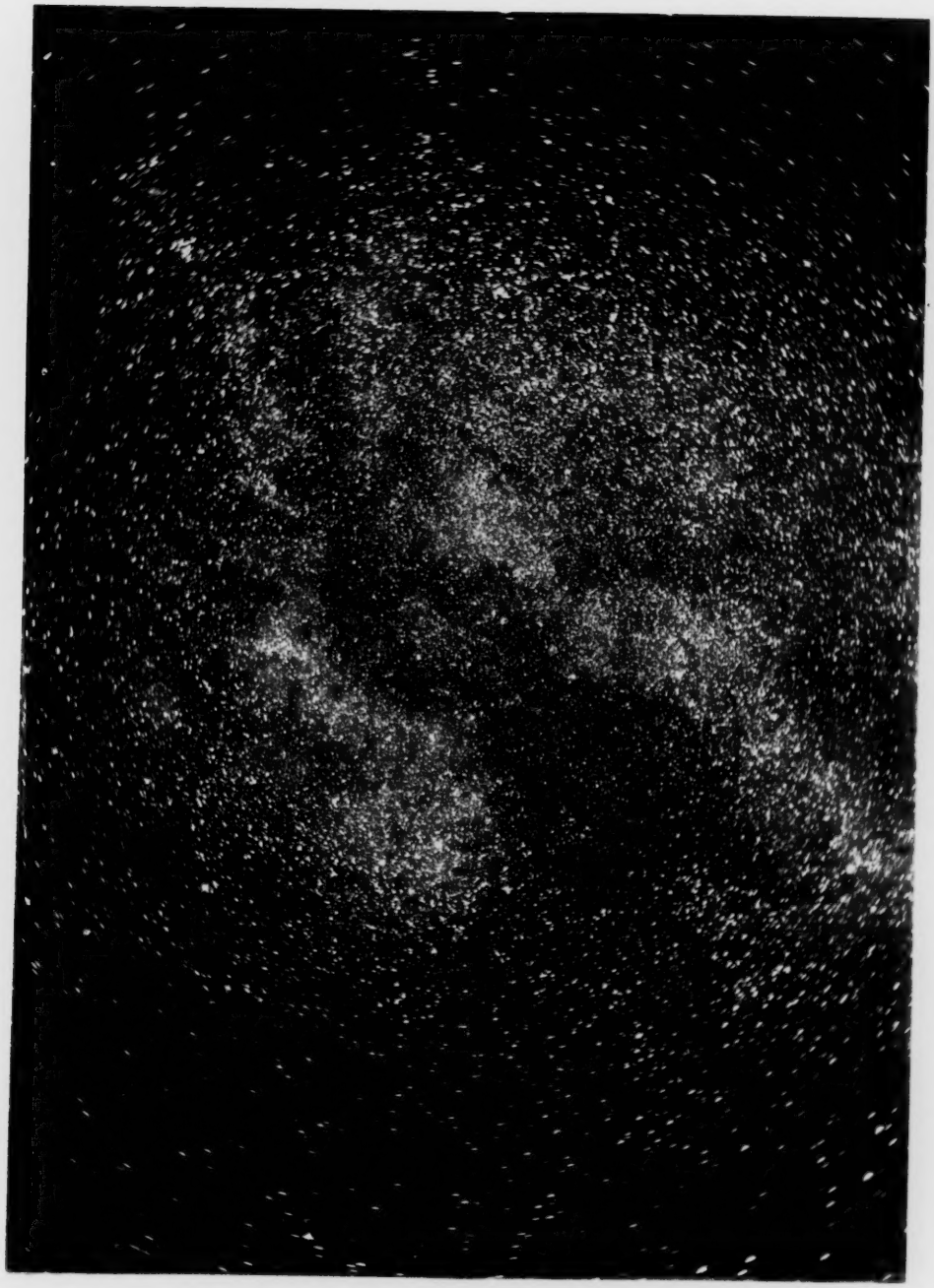

The Region of the Milky Way about ζ Cephei.

Enlarged from a photograph taken by Dr. Max Wolf, of Heidelberg, with a portrait lens of 55 millimètres (about $2\frac{1}{8}$ inches) aperture, and 195 millimètres (about $7\frac{5}{8}$ inches) focal length; exposure 7 hours.

The Automatic Engraving Company, Strode Road, Willesden Green.

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atmosphere, which consists mainly of permanent gases whose quantity is not altered by evaporation or liquefaction. With this is joined a much smaller atmosphere of aqueous vapour, which varies greatly with the conditions of time and place. But these variations would be much greater if the permanent gases were removed; for I think there is no doubt that their presence retards both evaporation and precipitation of vapour.

When a star is occulted by the moon, the disappearance is usually instantaneous; but to this rule there are many exceptions. It seems to me that both rule and exceptions can be explained by ascribing to the moon an atmosphere of vapour which never attains large dimensions, and is at many times and places wholly, or almost wholly, condensed. It need not, of course, be aqueous vapour. Oxygen and nitrogen would probably act in the same way if the temperature were sufficiently low, but it is not likely to be low enough for this purpose on the moon. The absence of cloud may be considered an objection to the theory of an atmosphere of vapour. But, as you pointed out in the last number, dust has a good deal to do with the formation of cloud, and so I believe has atmospheric air. In the absence of air and dust, precipitation would perhaps take place with little or no formation of clouds or fog.

The existence of water on the moon is doubtful. On Mars, however, it appears to be certain, and it is not unlikely that vapour plays a much more important part in the Martian atmosphere than in ours. On one point, however, we are at present wholly in the dark, viz., What is the effect of a diminution in the force of gravity upon water? We have only the power of studying this to a very limited extent on the earth, and I am not aware that it has been studied at all. From the marked effects of pressure on both the freezing and the boiling points of water it seems probable that variations in the force of gravity would affect these points also; but how, we can only conjecture. Water, however, might exist in the liquid state at a temperature far below freezing point if it were sufficiently salt. Again, supposing that Mars has no atmosphere of oxygen and nitrogen, the result would be to modify the properties of water considerably. Water when deprived of air becomes viscous, its boiling point is raised and it boils with explosive violence; even its visible appearance is altered.

Aqueous vapour, according to the experiments of Tyndall, has a very high absorptive power. This might account for the small reflection of light from the surface of Mars. If, moreover, it absorbs blue or green rays by preference, the red tinge of those reflected from the solid body of the planet would be accounted for. The red tinge of the eclipsed moon has sometimes been ascribed to a similar cause, the absorbing agent being the vapour in our own atmosphere. The atmosphere would probably be more dense in the equatorial than in the polar regions, but this would be modified by the distribution of water on the planet and the existence of mountains or high table-lands.

With regard to the *relative* albedo of different planets, it ought, I think, to be easy to detect. Supposing that the entire surface of the planet is illuminated (otherwise allowance can be made), the illuminated surfaces are proportional to the squares of the planets' diameters. If the whole of the incident light was reflected the intensity, for different planets, would vary directly as this square, and inversely as the square of the planet's distance from the sun multiplied by the square of its distance from us. If the relative lights thus computed for Jupiter and Mars differ from those directly observed, the difference is due to the relative albedo of the two planets. One planet should be taken as the standard and the albedo of each of the others expressed in terms of its albedo.

Truly yours,

16, Earlsfort Terrace, Dublin. W. H. S. Monck.

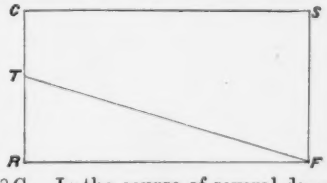
[The presence of air or other gases makes no difference in the amount of water that will be evaporated at a given temperature, but it makes a considerable difference in the rate of evaporation. Thus, if a pint and a half of water were brought into a room measuring ten feet each way, and filled with perfectly dry air, or any other gas or vapour, at a temperature of 80° Fahrenheit nearly the whole of the water would be evaporated, and the air would still remain transparent, and the walls of the room dry. When this amount of water had been evaporated the process would stop, and no more vapour would be given off unless the temperature of the room was raised. Experiments show that the same amount of water would be evaporated, at the same temperature, whatever amount of dry air there was in the room to start with; even though the room was devoid of air, the same amount of water would disappear, and the pressure of the resulting water vapour would raise a column of mercury in a barometer by about an inch. On Mars the same tension of water vapour would raise the column of mercury a little more than two and a half inches. I do not feel as sure as Mr. Monck or my friend Mr. Maunder as to the evidence with regard to the absorption of the sun's light by aqueous vapour in the atmosphere of Mars, and I am quite prepared to believe that the white polar caps of Mars are due to the white snow-like crystals of carbonic acid gas, or even to condensed atmospheric air.—A. C. Ranyard.]

CAN A PLANET ABSORB ITS OCEANS WITHOUT HAVING ITS SURFACE TEMPERATURE LOWERED?

To the Editor of Knowledge.

Sir,—In the June number of Knowledge, p. 114, the editor criticizes the suggestion that if Mars formerly had more extensive oceans than now, owing to internal cooling they might have been absorbed. He makes the point that in the case of the earth such an absorption could only occur in case the surface temperature, even in the equatorial regions, were reduced, owing to internal cooling, below the freezing point.

Now it seems to me that the internal temperature of the earth has no influence whatever upon our climate, and that if its internal temperature should be reduced to that of interplanetary space our surface temperature would still be practically the same as at present. Let us imagine an iron bar one thousand feet long and several feet in diameter, surrounded by some perfectly non-conducting substance. Let one end, CR, be maintained at a constant temperature of 1000° C., and let the other end, SF, be exposed to the outside air, which we will say has a mean temperature of 0° C. In the course of several days variations of temperature inside the bar will cease, and the temperatures at varying distances from CR can be represented by the ordinates of the line TF. Thus, at one-tenth of the distance from F to R, that is at one hundred feet distance, the temperature will be 100° very nearly.



If, instead of iron, the bar were made of clay we should have practically the same condition of affairs, only instead

of having to wait several days for a permanent condition of temperature to be established, we should have to wait several centuries. There would then still be one other difference between the two cases. Owing to the high conductivity of the iron, the surface SF would be a few degrees above the temperature of the outside air, while the surface of the clay would be almost exactly equal to it. This difference would depend upon the radiating capacity of the surface SF. In this respect our earth resembles the clay; in other words, the radiating and absorbing power of the surface SF so far exceeds the conductivity of the long bar of clay that the temperature of the surface depends practically entirely upon this radiating and absorbing power, and will be the same whether the temperature of CR is 0° C. or 1000° C.

Suppose now that the temperature of CR should be permitted to fall to 500°. Since the temperature of SF still remains at 0°, we should now be able to advance twice as far from F before we should reach the temperature of boiling water.

Applying this experiment to the case of our earth, let us assume that half of the free water of the planet is at present underground, and that all the microscopic cavities that are not too hot are already filled with water. When the internal temperature of the earth is lowered to one-half of its present figure, there will, other things being equal, be nearly twice as much space for the underground water, and as a result we may expect that our oceans will disappear, leaving our earth a warm but arid desert. Indeed, this might occur at an earlier date, since at the greater depths water would remain in the liquid form at a higher temperature.

William H. Pickering.

Arequipa, Peru,
August 5th, 1892.

P.S.—In printing my letter in the June number the word *if* has been left out; it makes a very material difference in my meaning. What I wrote was, "In the case of this planet (Mars), however, we have good reason for thinking that *if* it formerly had extensive oceans upon its surface, *that by* the gradual cooling to which it has been subjected there has been room formed for them in its interior."

[I agree with Prof. Pickering as to the increase in underground temperature being probably slower in the body of Mars than in the earth.

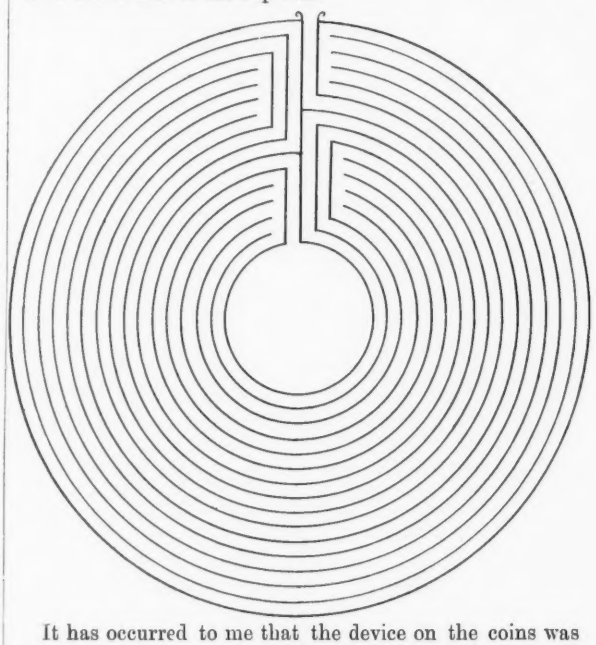
The great range in the mean temperature in passing from the earth's equatorial to its polar regions shows that if it were not for heat derived from the outside the whole surface of the earth would have a mean temperature below the freezing point of water. No amount of atmosphere could increase the heat of the surface derived directly* from the sun. One might as well expect to hasten the cooking of a joint before a fire by covering the joint with a glass shade or with a blanket, and we may be sure that the heat derived from the outside by the surface of Mars is, area for area, less than that derived by the earth's surface from the sun in a proportion which varies with the inverse squares of the distances of the two planets from the sun. If, therefore, as Prof. W. H. Pickering assumes, "the internal temperature of the earth has *no influence whatever* upon our climate, the mean temperature of even the equatorial regions of Mars must be far below the freezing point of water.—A. C. Ranyard.]

* Though an atmosphere which was transparent to short wave-lengths but absorbed long-wave lengths would tend to accumulate the heat of the surface by preventing the return from the earth's surface into space of long wave-lengths, corresponding to heat of low temperature derived from solar radiation of shorter wave-length.

THE CRETAN LABYRINTH.

To the Editor of Knowledge.

Dear Sir,—That there was some kind of a maze or labyrinth at Cnossus in Crete need not be doubted, as there are coins or tokens of Crete with the device of a labyrinth impressed upon them. Several of these coins are displayed in the glass cases accessible to the public in the British Museum. There is also, of course, the myth about Theseus entering the labyrinth by the help of a thread given him by Ariadne, when he slew the Minotaur or Man-Bull who lived in the centre, and put an end to the cruelties practised by the monster. This legend, beautiful enough when taken in the anagogic sense, was probably in existence before the maze shown on the coins took any definite shape, or it may be that several successive mazes were built and kept as long as any profit could be made out of those who came to visit them. I mention this because some of the mazes shown on the coins are circular, while others are rectangular, although the twists and turns are practically the same in both plans. Now, unfortunately, the labyrinths depicted on the coins are apparently of such a simple character that anyone could find his way to the centre and out again by the simple process of following his own nose, because the maze seems to consist of a roundabout path, leaving at no point any choice of routes to the explorer.



It has occurred to me that the device on the coins was never intended as a map or plan of the labyrinth, but only as a key or clue to the right path, leaving out the wrong ones. By taking the maze as shown on the Cretan coins, and treating the circular dividing walls as *double*, and each containing a passage of the same width as the road shown on the coins—in fact restoring the design to that shown on the diagram accompanying this paper—one can see that penetrating the maze to the very centre by the nearest road and at the first attempt would be a matter of very serious difficulty. Some of your younger readers will find it not quite easy to trace the white path with the point of a bone knitting needle; or, if they take the trouble to trace the maze large on the sea sand, they will find that their companions not in the secret will get a good deal of exercise before arriving at the centre. They must remember

too that they have the advantage of a bird's-eye view, which anyone entering a real structure made of walls or hedges would not.

I have at the principal doubling points merely left open two more of the roads than would have resulted from simply treating the dividing wall as another path. Though not necessary, this seems to give a little more variety to the plan.

It will be seen that there are four places where the right way has to be chosen between five, and as no one would be so stupid as to choose the road by which he had just entered, the chances are about three to one against his taking the right one of the other four. This difficulty is repeated four times in the course of the labyrinth, so that it would seem on a first calculation that his probability of getting in by the best route is about $\frac{1}{3}$ of $\frac{1}{3}$ of $\frac{1}{3}$ of $\frac{1}{3}$, or, say, 1 to 81. But any road will, after an uncertain number of turns and doublings, take him either to the centre or back again to the entrance; so that the task is not nearly so difficult as it would at first sight seem, nor, indeed, is it at all probable that the designer would have made it too difficult, especially if any body of men had to thrive on the profits, like the artificers who made silver images of Diana at Ephesus. Perhaps even the labyrinth coins took their origin from tokens, given to those who had visited the maze and duly paid for the privilege. They might also serve as a sort of pass token and souvenir of the visit, as well as a key to the mystery. The legend of the coloured thread of Ariadne becomes intelligible on this plan of the maze as now shown, because, on returning to any place where he had been before, Theseus would see his own thread and retrace his steps, gathering up the line until he could make a better start—while of course the thread would serve him as a guide to enable him to get out again. If I gave the coin plan, it would simply be each alternate circle of this one, and it would furnish a key by which anyone could immediately find the way to the centre.

The solution, when put into words, is—avoid every alternate circle or path.

Yours faithfully, Richard Inwards.

A FLAKE OF FLINT AND ITS HISTORY.

By R. Lydekker, B.A.Cantab.

In our article entitled "A Lump of Chalk and its Lessons," published in Knowledge for June last, it was stated that the upper white chalk, so far as hand specimens are concerned, is a nearly pure limestone. This, indeed, is a perfectly true statement as regards such hand specimens; but when we consider the upper chalk as a whole, we must not omit to regard the numerous bands and nodules of flint with which it is interstratified as a very important constituent of the whole rock. We say a constituent of the whole rock advisedly, because although the flint is now separated from the white limestone which we call chalk in the form of nodules and bands, yet there is evidence that it was originally disseminated throughout the entire mass, and that the upper chalk then formed a slightly siliceous limestone. Probably everybody is more or less familiar with flint as it occurs in a chalk pit, or in the form of gravel derived from the disintegration of chalk strata; but it may be taken for granted that comparatively few have ever seriously considered how the solid masses of flint have originated in the soft chalk limestone. As this is a subject of considerable interest, and one which has given rise to much discussion, we propose to devote the greater part of the present article to its consideration, while we shall add some observations on the history of flints after they have been removed from their native chalk.

We shall assume, in the first place, that all our readers are aware that flint is one of the manifold forms assumed by that abundant constituent of the earth's crust technically known as silica—the oxide of the element silicon. When crystallized, silica occurs in the form of rock-crystal, or quartz; but flint is one of the many non-crystalline, or amorphous, developments of the mineral. It is generally defined as a massive dark-coloured or black, semi-translucent, dull-looking variety of silica; which when pure burns to an opaque white, and has what is termed a conchoidal or shell-like fracture. The dark colour, it may be added, is due to the presence of a small quantity of organic matter, or carbon.

If we fracture a nodule of flint freshly taken from a chalk pit, we shall find that the thin edges of the sharp flakes which are seen to be produced have a pale-brown horn colour when viewed by transmitted light, and that as the flake becomes thicker the colour gradually darkens till it assumes the blue-black tint characteristic of the mass. If, however, our flake contains a portion of the external coat of the nodule, we shall see that for about a quarter of an inch in depth the outer layer is far less compact than the rest of the flint, and is of an opaque white colour. In other specimens, again, we shall observe that the colour, instead of the usual deep blue-black, is of a pale whitish blue, frequently marked by more or less distinct bands. Needless to say that on trying to scratch our flint flake with a knife we shall signally fail, and if any result happens it will be that a thin film from the metal of the blade will be left on the stone at the point of contact.

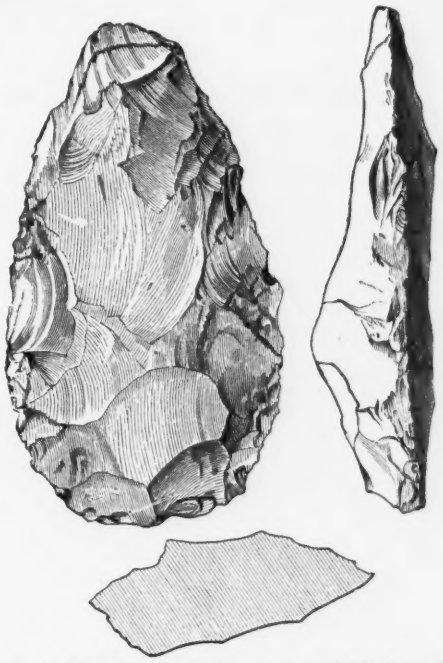


Fig. 1.—A Chipped Flint Implement, from Icklingham, half natural size. (From Sir J. Evans' "Stone Implements.")

Our flake will likewise exhibit in great perfection the characteristic conchoidal fracture of flint; that is to say, its surfaces will be smooth and undulating, swelling here into a prominent convexity, and falling there into a deeper or shallower hollow. Frequently, moreover, there may be observed a number of small parallel wavy ridges on the fractured surface. If we submit the edges of the freshly-

broken flake to a series of taps from our hammer, we shall find that a number of smaller flakes will be readily chipped off, each leaving a separate conchoidally-fractured surface on the original flake. It is this facility with which flint can be chipped, coupled with its hardness and the sharpness of its fractured edges, that induced our palæolithic ancestors to adopt it as the material for their various weapons and tools. The foregoing figure of one of these implements exhibits in great perfection the characteristic conchoidal fracture of flint.

The extreme development of this peculiar and characteristic fracture is, however, exhibited when a large flat surface of flint is struck at right angles by a round-ended hammer. The hammer then comes in contact with a minute portion of the surface of the flint, which may be represented by a small circle, and as the flint is elastic, "this small circle," as Sir John Evans observes, "is driven slightly inwards into the body of the flint, and the result is that a circular fissure is produced between that part of the flint which is condensed for the moment by the blow, and that part which is left untouched. As each particle in the small circle on which the hammer impinges may be considered to rest on more than one other particle, it is evident that the circular fissure, as it descends into the body of the flint, will have a tendency to enlarge in diameter, so that the piece of flint it includes will be of conical form, the small circle struck by the hammer forming the slightly truncated apex." A little practice will enable anyone to make these flint cones with ease. The size of the cone, and the degree of steepness of its sides, vary with the nature of the flint, the weight and form of the hammer, and the force of the blow.

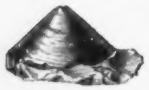


Fig. 2.—Artificial Cone of Flint. (From Sir J. Evans.)*

When examined with a lens or microscope, chalk-flint frequently exhibits a perfectly uniform structure throughout, without the least trace of the presence of any organic body. In other cases, however, traces of sponges, corals, shells, echinoderms, diatoms, &c., are more or less apparent in flint. Perhaps the most common of all these organisms are the mushroom-shaped sponges known as ventriculites; and if we examine a flint containing one of these sponges we shall frequently observe that there is a complete transition from portions of the perfectly preserved sponge to homogeneous flint, without the least trace of organic structure. In the case of echinoderms, we shall find that while in some cases the whole interior of the shell (or, as it is technically called, test) is filled with flint, the shell itself retaining its original calcareous structure, in other instances the original shell itself has been completely removed and replaced by flint. In some cases the original structure of the shell has been preserved in the flint, but more generally this has been completely lost, and the flint is structureless.

This replacement of a calcareous by a siliceous structure is an instance of pseudomorphism. Similar remarks will apply to the shells of molluscs, and likewise to corals. It is important to mention that the sponges found in flint were not originally of the horny nature of our bath-sponges, but were themselves composed of minute spicules and fibres of silica, like the so-called Venus's flower-basket of our modern seas.

With regard to the mode of occurrence of flint, we have first to mention that it is by no means confined to the chalk, but may occur in limestones of any age. In this country it is, however, more abundant and purer in the chalk than in any other formation, and may, indeed, be considered characteristic of the upper part of that formation.† Flints are found in the chalk either in the form of nodules or in thin continuous laminæ. The nodules are generally of very irregular shape, and may vary in size from a walnut to masses of a hundred-weight or so. As a rule they occur in strings at comparatively regular intervals in the chalk, generally conforming more or less closely to the original planes of bedding, and the individual nodules being sometimes at considerable distances apart, but at others closer together and more or less connected by long root-like pieces. On the other hand, the laminated or tabular flint may cut the bedding-planes of the chalk at any angle, and is often found in joints and fissures, which may emerge at the surface. As a rule, this tabular flint is devoid of organisms. Not unfrequently flints may be found which near their surfaces gradually become paler and paler in colour, and contain an increasing amount of calcareous matter, till they pass imperceptibly into hard siliceous chalk. Again, flints may be hollow, and contain in their cavities either corals or sponges, or masses of that variety of silica known as chalcedony. The latter, it may be mentioned, is a semi-transparent waxy-looking stone, generally with a more or less decided pinkish tinge, and forming mammillated or botryoidal masses. The milky and reddish varieties of chalcedony constitute carnelian, while when it is arranged in differently coloured bands it forms agate. Occasionally small crystals of quartz occur in the hollows of flint.

It has still to be mentioned that in some districts—and more especially near Norwich—in addition to the horizontal layers of nodular flints, there occur in the upper chalk a number of huge cup-shaped masses of flint placed one above another in vertical lines; these masses being locally known as "potstones," and presenting a remarkable resemblance to certain giant sponges called Neptune's cups.

The proportion of the flint to the chalk in the upper chalk of England varies, according to Prof. Prestwich, from four to six per cent. It is important to add that the masses of nodular flint may not unfrequently be found to be traversed by fractures which have subsequently been reunited; thus showing either that the substance must at the time have been in a semi-plastic condition and capable of reunion, or that the fracture has been united by the subsequent deposition of siliceous matter. It must also be mentioned that, as a rule, the white coating is confined to the outer surface of the nodules, and that we do not find layers of pure flint overlain with white coats and then again by other similar layers; thus indicating that the formation of the white coat was the final act in the development of flint.

With these observations on the structure and mode of occurrence of flint, we are in a position to enter on the more difficult subject of its origin. From the very first it was recognized by all geologists that such a peculiarly hard and homogeneous substance as flint, occurring in irregular nodules among the pure white chalk, could not have been deposited in its present condition directly from the waters of the cretaceous sea; and the problem therefore presented itself to account adequately for its mode of formation. The problem soon resolved itself first of all into two questions, namely, whether the silica was originally part and parcel of the chalk as first deposited and that it

* We are indebted to Messrs. Longman and Green for this and the figure on the previous page.

† It also occurs abundantly in the lower part of the Portland stone series of the Isle of Portland, but is there generally less pure, and has the conchoidal fracture less marked.

subsequently gained its present condition, or whether it was added at certain intervals during the deposition of the chalk from a totally different source, or was introduced subsequently to the deposition of the whole.

I believe I am right in saying that there is still a current notion among many persons who are not scientific geologists that flint is a kind of igneous rock; and its hardness and superficial resemblance to some kinds of obsidian may at first sight lend some countenance to such an idea. Nevertheless, it is almost superfluous to add, such a notion is a totally erroneous one, and the mode in which flint occurs—without altering the limestone with which it is in contact, or the fossils which it contains—is of itself sufficient to refute any such origin.

From the hardness and insolubility of flint, it would appear a very natural inference that silica in all forms is likewise insoluble, but, as proved by siliceous springs, silica in a certain condition is freely soluble in alkaline waters. This naturally suggested to the earlier geologists that flints had been deposited by the aid of hot springs or heated waters in the cretaceous sea at certain intervals during the formation of the chalk sea. Thus, the late Dr. Mantell wrote that "the nodules and veins of flints that are so abundant in the upper chalk have probably been produced by the agency of heated waters holding silex in solution, and depositing it when poured into the chalk sea." It will, however, be obvious that there are very serious difficulties in accepting any such explanation. In the first place, we should require the introduction of floods of heated water containing silica at certain irregular intervals during the whole period of the deposition of the upper chalk; and it would also be essential that these waters should have extended over vast areas of ocean. Secondly, the mode in which nodular flint occurs could not adequately be explained by any theory of this kind; while it would leave the origin of the tabular flint, which we have seen may occur in joints and fissures, totally unaccounted for. Another idea was that the silica had been, at least partially, introduced from above after the deposition of the chalk; but, although this explanation may, and perhaps does, partly account for the formation of some of the upper tabular flints, it is quite impossible that it could explain the formation of the numerous layers of nodular flints throughout the body of the chalk.

There is, however, an explanation which will readily account for all the features presented by the chalk-flints, and requires the aid of no foreign factors in the process. This explanation is based on the phenomenon known as segregation. Now segregation is the tendency presented by a small quantity of one substance, when diffused through a much larger quantity of another substance, to collect together in nodules or strings, which generally accumulate either around some fragment of their own nature or some foreign body—especially an organic one—as a nucleus. We have well-known examples of this segregating process in the huge lenticular calcareous masses termed "septaria," found in the London and Kimeridge clays, and also in the iron-nodules of other formations. Premising that soluble silica has a peculiar affinity not only for any kind of silica, but likewise for gelatinous organic substances (both of which were presented by the sponges of the cretaceous seas), it will be obvious that if we can only satisfy ourselves that at the time of its deposition the chalk contained diffused among its substance a sufficient amount of soluble silica, we shall at once be able to account for the formation of its flint. Now, as we pass upwards in the cretaceous system from the lower greensand to the upper chalk, we find a gradual change from a completely siliceous to a calcareous rock. Moreover, while in the gault, upper greensand, and lower chalk (in which in the south of England there are no flints) there is a large but decreasing amount of soluble silica, varying from 46 per cent. in the upper greensand to 31 per cent. in the chalk-marl, when we reach the upper chalk with flints such soluble silica is reduced to a mere trace. As it is at the base of the white chalk that the sponges attain their greatest development, and as it is also here that flints first commence, the disappearance of the soluble silica may be safely attributed to its segregation by means of the sponges and other bodies, and its conversion into flint. Prof. Prestwich, writing on this subject, observes that in presence of the siliceous spicules of the cretaceous sponges and their gelatinous animal matter, "the soluble or colloidal silica, dispersed through the soft chalk-mud, slowly segregated from out of the surrounding pulpy mass, and gradually replaced part or the whole of the organic matter, as it decayed away. Nor has it stopped there; owing to the affinity of the particles of colloidal silica amongst themselves, the segregation has not ceased with the replacement of the organic body, but has continued so long as any portion of silica remained in the surrounding soft matrix; whence the frequent excess of flint beyond the interior or body of the shells, echinoderms, &c., and whence also the irregular shape arising from this overgrowth of the flint nodules." Next to sponges, echinoderms seem to have afforded the most attractive centres of segregation; and while in some cases only their shells have been filled with flint, in other instances we find a mass of these shells cemented together by a nodule of flint.

In some parts of the Continent, and also in Yorkshire, we find that for some reason or other—not improbably a greater development of sponges and a smaller amount of soluble silica—the segregating process has extended downwards to the lower chalk, where flints are then found; and we suspect that in such cases analysis would also show in these beds a corresponding absence of free soluble silica.

With regard to the so-called "potstones" of the Norfolk chalk, some of which may be upwards of a yard in height, with a diameter of a foot or so, the only adequate explanation of their formation that has yet been offered is that they represent gigantic cup-like sponges which have grown one upon the top of the other, as they were successively buried in the newly-formed chalk, and that they have been subsequently silicified by the same segregating process.

We conclude, therefore, that the flints of the chalk were originally an integral portion of the rock itself, which was then a slightly silicated limestone; and that the present purely calcareous character of the chalk is due to the separation of the silica by segregation. We have, however, still to account for the relatively large amount of soluble silica present in the cretaceous rocks, since this is far in excess of what would have been brought down by most rivers of the present day, in the waters of which the amount of this substance is almost infinitesimal. It has been suggested that the unusual supply may have been afforded by the cretaceous rivers being largely fed by siliceous springs; but although this may have been one factor in the case, a more probable theory is that the drainage area supplying the cretaceous sea with sediment was largely composed of decomposed felspathic rocks, in which the amount of this silica would have been amply sufficient to have furnished the quantity present in the chalk.

It is, however, not only with regard to its mode of origin that flint is of more than ordinary interest. Being an excessively hard substance, it is one exceedingly difficult to be worn to powder by the action of water, and the

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flint gravels of the valleys of the south of England, as well as the beaches of our southern coasts, and the numerous tertiary deposits composed of flint pebbles, remain to us as silent witnesses of the vast denudation of the upper chalk which has taken place in this country. Remembering that the proportion of flint to chalk is only from four to six per cent., and also bearing in mind that all the flints in our gravels have been considerably reduced in size by the action of water, we may fairly say that every cubic yard of pure flint gravel represents the removal of at the very least twenty cubic yards of pure chalk; and to this we have to add all the lower chalk which has been denuded without leaving any solid residue. Moreover, when we recollect that this denuding process has been going on ever since the eocene period, and that our river gravels only represent a small portion of the flints left by the denudation of the chalk during the latter part of this protracted period of time, we may gain some faint conception of how enormous this denudation must have been.

Although the flint gravels of our rivers afford some estimate, however faint, of the denudation of the chalk during the pleistocene period, it would be quite incorrect to assume that the flint pebbles forming the beaches of our southern coasts present a record of the amount of denudation which has taken place during the modern period. We have already mentioned that a freshly-broken flint presents a uniform blackish-blue colour throughout its interior, and any flint pebble on the seashore which had been recently derived from its native chalk would, when broken, present a similar appearance. As a matter of fact we shall find, however, that at least ninety per cent. of such pebbles are stained yellow, brown, red, or black internally, and as most of the flint fragments in many of our older gravels are likewise similarly stained, we shall have little hesitation in coming to the conclusion that our modern sea-beaches are largely derived from the breaking up of such old gravel beds, and the subsequent rounding of these irregular fragments of flint into pebbles by the action of the sea. The staining of the flints is of course due to the large amount of ferruginous matter contained in the gravels, and owing to the banded nature of the original flint it frequently gives rise to an agate-like appearance in the pebbles. Many of the pebbles in our beaches are, however, derived from still older sea-beaches, like the one now remaining at the southern extremity of the Isle of Portland, while others, again, owe their origin to the breaking up of the eocene Woolwich and Reading beds, which are largely composed of flint pebbles. Sometimes, indeed, fragments of these old beds also occur in the river gravels, where blocks of the Hertfordshire conglomerate—the equivalent of the Woolwich and Reading beds—may be met with.

We have thus abundant evidence of the exceeding indestructibility of flint, and how it may go on from one formation to another to tell, when rightly interpreted, the various steps in the denudation of our country.

In addition to being frequently stained internally, the observer will also not fail to notice that all of the flints in our river gravels have acquired a white or yellow porcelaneous external coating, quite different from the interior; and it is believed by Sir John Evans that this white coating has been produced by the removal of a portion of the flint which was still soluble "by the passage of infiltrating water through the body of the flint." That such a process must have been of inconceivable slowness, and must have required countless years for its accomplishment, goes without saying. We have, indeed, some inkling of how extremely slow this process must be, by the circumstance that the fractured surfaces of the flints built into the walls of our very oldest churches show not the slightest change from their pristine condition. When, however, we examine the chipped flint implements of our river gravels and caves, like the one shown in our first illustration, we find their surfaces altered precisely in the same manner as the flint fragments by which they are accompanied. Hence we gain, from a totally independent source, some idea of the immense antiquity of the period when the old palæolithic hunters inhabited the south of England.

Having thus reached the subject of flint implements, we feel tempted to enter into the consideration of some of their different types and the beds in which they occur, but editorial limitations of space forbid our wandering into such entrancing paths. Even, however, without entering into this part of the subject, we trust that what we have written will serve to show that a "Flake of Flint," when considered from all points of view, is to the full as interesting as a "Lump of Chalk."

THE FACE OF THE SKY FOR OCTOBER.

By Herbert Sadler, F.R.A.S.

THERE does not seem to be any visible diminution in the number of spots and faculæ on the solar disc. There will be an eclipse of the Sun, visible over a great part of North America, on the 20th, $\frac{91}{100}$ths of the disc being obscured. Conveniently observable minima of Algol occur at 10h. 10m. P.M. on the 14th, and 6h. 58m. P.M. on the 17th. The Nova Aurigæ has made a brief reappearance, but appears to be sinking in magnitude.

Mercury is too near the Sun to be visible in October, being in superior conjunction on the 8th. Venus is a morning star, and is still well placed for observation, although her brightness at the end of the month is only about one-half of what it was at the beginning of June, and her diameter is also decreasing notably. She rises on the 1st at 1h. 46m. A.M., with a northern declination of 12° 50½′, and an apparent diameter of 20½″, $\frac{56}{100}$ths of the disc being illuminated. On the 17th she rises at 2h. 19m. A.M., with a northern declination of 7° 44′, and an apparent diameter of 18″, $\frac{64}{100}$ths of the disc being illuminated. On the 31st she rises at 2h. 49m. A.M., with a northern declination of 2° 10′, and an apparent diameter of 16″, $\frac{7}{10}$ths of the disc being illuminated. During the month she passes through Leo into Virgo, being near Regulus on the 5th.

Mars is an evening star, and is better situated as to his elevation above the horizon, though his apparent diameter has markedly decreased, and, of course, his brightness also—at the end of October being only a quarter of what it was at opposition. He rises on the 1st at 4h. 10m. P.M., with a southern declination of 20° 59′, and an apparent diameter of 16·6″, the defect of illumination on the following limb being very marked. On the 31st he rises at 2h. 30m. P.M., with a southern declination of 15° 13′, and an apparent diameter of 12½″, the gibbous form of the planet being very apparent. During the month he is in Capricornus, and makes an exceedingly near approach to the third magnitude star δ Capricorni on the 25th. Unfortunately, at the time of nearest appulse, Mars will be below the horizon in England.

Jupiter is a superb object in the evening sky, and, as we remarked in the "Face of the Sky for September," is visible to the naked eye in sunlight, the present opposition of the planet being one of the very favourable ones which occur once every twelve years. Jupiter rises on the 1st at

5h. 59m. P.M., with a northern declination of 6° 50′, and an apparent equatorial diameter of 49·1″. On the 12th he rises at 5h. 11m. P.M., with a northern declination of 6° 17′, and an apparent equatorial diameter of 49¼″. This is the day of opposition, the distance of Jupiter from the earth being 367¼ millions of miles. On the 31st he sets at 5h. 6m. A.M., with a northern declination of 5° 23′, and an apparent equatorial diameter of 48½″. He is occulted by the Moon on the evening of the 6th (not visible in England), and the same evening a 9½ magnitude star will be occulted by the planet, the time of central occultation being about 8h. P.M. During the month he describes a retrograde path in Pisces. The following phenomena of the satellites occur while Jupiter is more than 8° above and the Sun 8° below the horizon. On the 1st a transit ingress of the shadow of the third satellite at 9h. 47m. P.M., and a transit ingress of the third satellite at 11h. 13m. P.M. On the 3rd a transit ingress of the shadow of the second satellite at 7h. 39m. P.M., of the satellite itself at 8h. 11m. P.M.; a transit ingress of the shadow of the first satellite at 9h. 27m. P.M., of the satellite itself at 9h. 42m. P.M.; a transit egress of the shadow of the second satellite at 10h. 12m. P.M., of the satellite itself at 10h. 36m. P.M.; a transit egress of the shadow of the first satellite at 11h. 41m. P.M., of the satellite itself at 11h. 54m. P.M. On the 4th an eclipse disappearance of the first satellite at 6h. 41m. 58s. P.M., and an occultation reappearance of the same satellite at 9h. 5m. P.M. On the 10th a transit ingress of the shadow of the second satellite at 10h. 18m. P.M., and of the satellite itself at 10h. 27m. P.M.; a transit ingress of the shadow of the first satellite at 11h. 21m. P.M., and of the satellite itself four minutes later. In this case and the next transit attention should be paid to a possible projection of the satellite on the shadow. On the 11th an eclipse disappearance of the first satellite at 8h. 37m. 5s. P.M., and its reappearance from occultation at 10h. 49m. P.M. On the 12th an occultation reappearance of the second satellite at 7h. 29m. P.M.; a transit egress of the shadow of the first satellite at 8h. 3m. P.M., and of the satellite itself one minute later. On the 18th an occultation disappearance of the first satellite at 10h. 21m. P.M. On the 19th an occultation disappearance of the third satellite at 7h. 8m. P.M., of the second at 7h. 15m. P.M.; a transit ingress of the first satellite at 7h. 34m. P.M., and of its shadow at 7h. 46m. P.M.; a transit egress of the first satellite at 9h. 46m. P.M.; an eclipse reappearance of the third satellite at 9h. 53m. 46s.; a transit egress of the shadow of the first satellite at 9h. 58m., and an eclipse reappearance of the second satellite at 10h. 3m. 7s. It will be observed that for some time this evening, as on the 26th of the month, Jupiter will appear attended only by the fourth satellite. On the 20th an eclipse reappearance of the first satellite at 7h. 10m. 23s. P.M. On the 26th a transit ingress of the first satellite at 9h. 18m. P.M., of its shadow at 9h. 39m. P.M., an occultation disappearance of the second satellite at 9h. 28m. P.M., an occultation disappearance of the third satellite at 10h. 23m. P.M.; a transit egress of the first satellite at 11h. 30m. P.M., and of its shadow at 11h. 53m. P.M. On the 27th an occultation disappearance of the first satellite at 6h. 31m. P.M., and its reappearance from eclipse at 9h. 5m. 43s. P.M. On the 28th a transit egress of the first satellite at 5h. 56m. P.M., of its shadow at 6h. 22m. P.M.; a transit egress of the second satellite at 6h. 35m. P.M., and of its shadow at 7h. 26m. P.M.

Saturn does not rise on the last day of the month till 3h. 52m. A.M., and Uranus is in conjunction on the 29th. Neptune is an evening star, rising on the 1st at 8h. 0m. P.M., with a northern declination of 20° 34′, and an apparent diameter of 2·6″. On the 31st he rises at 6h. 2m. P.M., with a northern declination of 20° 29′. During the month he describes a short retrograde path to the N.E. of ϵ Tauri.

October is rather a favourable month for observations of shooting stars, the most marked shower being that of the Orionids, from the 17th to the 20th of the month, the radiant point being situated in 7h. 0m. R.A. and 15° north declination. The radiant point rises at the date named at about 8h. 45m. P.M., and sets shortly after 4 A.M.

The Moon is full (the Harvest Moon) at 6h. 12m. A.M. on the 6th; enters her last quarter at 9h. 37m. P.M. on the 12th; is new at 6h. 24m. P.M. on the 20th; and enters her first quarter at 9h. 26m. P.M. on the 28th. She is in perigee at 5·5h. A.M. on the 7th (distance from the earth 222,740 miles), and in apogee at 3·6h. A.M. on the 22nd (distance from the earth 252,590 miles). The greatest eastern librations take place at 0h. 7m. P.M. on the 2nd, and 8h. 8m. P.M. on the 29th, and the greatest western at 10h. 9m. P.M. on the 10th.

Chess Column.

By C. D. Locock, B.A.Oxon.

All communications for this column should be addressed to the "Chess Editor, *Knowledge Office*," and posted before the 10th of each month.

Solution of September Problem (by C. D. P. Hamilton).

1. B to Kt4, and mates next move.

Correct Solutions received from Betula, G. K. Ansell.

G. S. Cummings.—The criticism may be just, but was a little premature. Black answers 1. Q to Q4 by B to Bsq.

L. Bourne.—If, as you probably mean, 1. KB × KKtP, Black replies P to Kt8 becoming a Knight (ch), an effective resource.

A. G. Fellows.—Thanks for the two-mover, but your key-move fails against the defence 1. . . P to B4 (or 3); whereas the problem *is* solved by 1. P to B3ch, K moves; 2. P to Q4 mate. Perhaps, however, the diagram is incorrect? You speak of 9 White pieces, of which 8 only are visible.

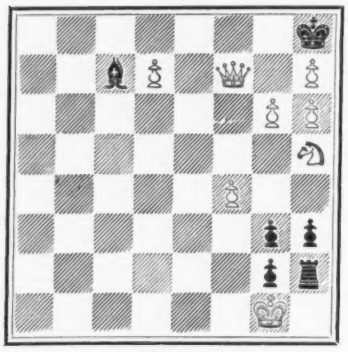
Betula.—Your last question is the easier to answer. Identical positions resulting from different key-moves must be regarded as practically one and the same problem. The answer to the other question must depend chiefly on (1) the number and (2) the importance of the similar variations. There can of course be no fixed rule: but assuming that a good two-mover contains four leading and more or less original variations A, B, C, D; and also about four minor variations, a, b, c, d, then a rough rule might be as follows: "If *none* of the leading variations are the same, any or all of the minor variations may occur; or if *one* of the leading variations is present (provided it is not the *chief* variation), one or two of the minor variations may occur." Thus, if the original problem is represented by A+B+C+D+a+b+c+d, I should say that E+F+G+H+a+b+c+d, if by the same composer, might be regarded as a different problem. So also C+E+F+G+a+b+e+f; *not*, however, A+E+F+G+e+f+g+h, where A is a *conspicuously leading* variation. No authority is claimed for this rule, and much, moreover, must depend on the key-move and general nature of the problem; but it seems fairly safe to consider the leading variations chiefly, neglecting the minor variations, unless of course these are greatly in excess of the others.

XUM

PROBLEM.

By G. K. Ansell.

Black.



White.

White to play, and compel Black to mate in two moves.

Appended is the third game of the recent Newcastle Match.

"Sicilian Defence."

White (E. Lasker).	Black (H. E. Bird).
1. P to K4	1. P to QB4
2. Kt to KB3	2. Kt to QB3
3. P to Q4	3. P × P
4. Kt × P	4. P to KKt3 (*a*)
5. Kt × Kt	5. KtP × Kt
6. Q to Q4	6. P to B3
7. Kt to B3	7. B to KKt2 (*b*)
8. B to BQ4	8. Kt to R3
9. Castles	9. Kt to B2
10. B to K3	10. Castles
11. Q to Q2 (*c*)	11. P to K3
12. KR to Qsq	12. Q to R4 (*d*)
13. QR to Ktsq	13. R to Ksq (*e*)
14. P to QKt4	14. Q to B2
15. B to B4	15. Kt to K4 (*f*)
16. B to QKt3	16. B to Bsq
17. P to Kt5	17. R to Ktsq
18. P to QR4	18. B to K2 (*g*)
19. B to Kt3	19. K to Kt2
20. P to B4	20. B to B4ch
21. K to Rsq	21. Kt to B2
22. P to B5	22. P to K4 (*h*)
23. B × Kt	23. K × B
24. Q to R6	24. P to Kt4
25. Q × Pch	25. K to Bsq
26. Q to R6ch	26. K to K2
27. Q to Kt7ch	27. K to Qsq
28. Q × Pch	28. B to K2
29. Q × KP	29. P to Q3
30. P to B6	30. B to Bsq
31. Q × KtP	31. Q to B2
32. B × P	32. B × B
33. R × Bch	33. K to B2
34. Q to QB5	34. R to Kt3
35. Kt to Q5ch	35. K to Kt2
36. P × Pch	Resigns.

Notes.

(*a*) Not good here. The move should have been made, if at all, at move 2. On his next move he would do better, perhaps, to retake with the QP.

(*b*) 7. . . Q to Kt3 would not be good. The White Queen would play to B4, followed by Kt to R4 sooner or later. Owing to his faulty opening, Black experiences some difficulty in castling.

(*c*) The Black King's Bishop being now defended, the Queen does well to retire.

(*d*) A favourite move of Mr. Bird's. He cannot play 12. . . P to Q4 on account of 13. P × P, P × P; 14. Kt × P !, P × Kt.; 15. B × P, as Mr. Lasker points out.

(*e*) Probably to make room for the Bishop at Bsq, in view of the probable posting of the White QB at B5. Perhaps, however, 13. . . P to Q4 would be preferable.

(*f*) 15. . . P to K4 would not only pin the Knight, but leave his Queen's Pawn still weaker.

(*g*) He has not much to do, but P to QR3 seems more to the purpose.

(*h*) This loses the game, which was still defensible by 22. . . Kt to K4.

The remainder of the game is remarkable for the rapid disappearance of the Black Pawns.

CHESS INTELLIGENCE.

A short match played at Newcastle last month, between Messrs. H. E. Bird and E. Lasker, resulted in a not unexpected victory for the latter by five games to none. There were no drawn games, though some were well contested; Mr. Lasker being generally content to exchange Queens at the earliest opportunity, relying solely on his superior skill in the end-game.

A quadrangular match was in progress at Belfast last month. Mr. Lasker abandoned his intention of competing, Messrs. Mason and Lee taking his place. The score up to September 19th was as follows: Blackburne 3, Mason 3, Bird 3, Lee 1.

The *Chess Monthly* will in future be published by Mr. Horace Cox (Bream's Buildings, E.C.). The alteration is designed as a certain cure for the attack of unpunctuality from which the leading chess magazine has recently been suffering.

A match between Dr. Smith and Mr. T. Block, played recently at the City of London Chess Club, resulted, after a very hard fight, in a victory for Dr. Smith by five games to four.

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